Antiquity

A QUARTERLY REVIEW OF ARCHAEOLOGY

Edited by Glyn Daniel

Vol. XXXII No. 126 JUNE 1958

Editorial – – – – – – – – – – 65
Retrospect. By V. Gordon Childe – – – – – 69
The Dawn: and an Epilogue. By Stuart Piggott – – 75
The Corinthian Isthmus and the Isthmian Sanctuary. By Oscar Broneer – – – – – – – – – 80
The Nature of Wansdyke. By Anthony Clark – – – 89
The Linear A Question. By Maurice Pope – – – – 97
The Morley St Peter Hoard. By R. Rainbird Clarke and R. H. M. Dolley – 100
The Excavation of Two Long Barrows in Wessex. By Faith de M. Morgan and P. Ashbee – – – 104
Inscriptions in the Libyan Alphabet from Ghirza in Tripolitania. By J. M. Reynolds, Olwen Brogan and David Smith 112
Notes and News – – – – – – – – – 116
Reviews (*see overleaf*) – – – – – – – – 129
Book Chronicle – – – – – – – – – 79, 143

Published at Ashmore Green, Newbury, Berks., England

Price: Single Copies 7s 6d—Annual Subscription 30s. (or 5 dollars)

ADVISORY EDITORS

Professor Dr Gerhard Bersu, Dr G. H. S. Bushnell, Professor M. E. L. Mallowan, Professor Stuart Piggott, and Sir Mortimer Wheeler.

REVIEWS

Catalogue of Manuscripts containing Anglo-Saxon. By N. R. Ker (*Dorothy Whitelock*) – 129
Ancient Mycenae. By George E. Mylonas (*Lord William Taylour*) – – – – 132
Préhistoire de l'Afrique du Nord: Essai de Chronologie. By Lionel Balout (*Günter Smolla*) – 133
Ugaritica III. By Claude F.-A. Schaeffer (*O. R. Gurney*) – – – – – – 135
The Testimony of the Spade. By Geoffrey Bibby (*R. J. C. Atkinson*) – – – – 137
Corinth. Vol. XVI. By Robert L. Scranton (*D. Talbot Rice*) – – – – – 138
Greece before Homer: Ancient Chronology and Mythology. By Sir John Forsdyke (*John Boardman*) – – – – – – – – – – – – 138
Early Rome II: The Tombs. By E. Gjerstad (*John Boardman*) – – – – – 139
Studies in Ancient Technology. Vols. I to IV. By R. J. Forbes (*Garry Thomson*) – – 139
Les Fouilles d'Ishango. By J. de Heinzelin de Braucourt (*J. Desmond Clark*) – – – 141

EDITORIAL NOTICES

ANTIQUITY is published quarterly on the 1st of March, June, September and December.

The ANNUAL SUBSCRIPTION (including postage) is ONE POUND TEN SHILLINGS Sterling, or FIVE DOLLARS, *payable in advance to* ANTIQUITY, Ashmore Green, Newbury, Berks., England. SINGLE COPIES may be obtained for 7*s* 6*d* (post paid).

Everyone wishing to subscribe, or to obtain single copies, is asked to *write direct* to the Publisher, H. W. EDWARDS, Ashmore Green, Newbury, Berks., England. Tel. Thatcham 2105.

MSS (whether in the form of Articles or Notes) offered for publication should be sent to Dr GLYN DANIEL, St. John's College, Cambridge, England. All MSS should be typewritten. The Editor is not responsible for the opinions expressed by contributors.

Contributors can help the Editor:

1. By sending a neat and legible typescript.
2. By confining proof-corrections to mistakes. Alterations of the text can only be accepted in the most exceptional cases, e.g., the discovery or publication of new matter.
3. By returning the corrected proof at once to the Editor, by air-mail from overseas.

BOOKS for REVIEW should be addressed to the EDITOR OF ANTIQUITY, St John's College, Cambridge, England.

Offprints are not supplied except on demand; they may be bought from The Whitefriars Press Ltd., 26 Bloomsbury Way, London, W.C.1, if ordered at least three weeks before date of publication. Offprints of Notes are not supplied.

BINDING: John Bellows Ltd, Printers and Publishers of Eastgate, Gloucester, will continue to assist those wishing to bind their copies of ANTIQUITY. Binding cases only are 8*s* 6*d*; the cost of binding each volume is 15*s* (including case and return postage). Please write direct to John Bellows Ltd.

Antiquity

VOL. XXXII No. 126 JUNE 1958

Editorial

WE are privileged to print in this number of ANTIQUITY, with the kind permission and at the suggestion of Professor Grahame Clark (his literary executor), a remarkable document written by the late Professor Gordon Childe shortly before his sudden and universally lamented death in Australia last autumn. Childe has left us three posthumous documents: first *The Prehistory of European Society*, published in February of this year and which, with the sixth edition of *The Dawn of European Civilization*, is reviewed in the following pages by Professor Piggott; secondly, his final lecture to the Institute of Archaeology of the University of London (now moved to new premises at Gordon Square, W.C.1) and, thirdly, the autobiographical note which we publish here under the title of ' Retrospect '. Inaugural lectures are publicized and published, and we have before us Professor Ian Richmond's inaugural lecture as Professor of the Archaeology of the Roman Empire at Oxford, and Professor Dorothy Whitelock's inaugural lecture as Elrington and Bosworth Professor of Anglo-Saxon at Cambridge, and we await with interest the publication of the inaugural lectures of Professor Grimes and Professor J. D. Evans at London, Professor de Valera at Dublin, and Professor R. J. C. Atkinson at Cardiff. But valedictory lectures could be even more interesting than initial statements of hope and policy: we would think they should be compulsory. It is good to know that Gordon Childe's valedictory address will be published by the Institute of Archaeology of the University of London in what they now propose to call their *Bulletin* (old style *Annual Report*).

Childe's statement, which we are most happy to publish here, is more than a valedictory address; it is also, in a kind of way, an obituary. Sir Mortimer Wheeler has declared (ANTIQUITY XXXII, 1958, 4) that obituaries and epitaphs should be written ' during the fulness of its subject's life and work ', and this embarrassing practice, which would rapidly put the necrology offices of *The Times* and The British Academy out of business, may be a good thing. What is obviously impossible is that they should be done by the subject when he is full of life and work. No man is wise enough to write his own obituary. He knows too well his three conflicting personalities, the self he thinks himself to be, the self—the *persona*—that he projects to the world, and thirdly (this he knows so badly) the self which the world sees. Childe's ' Retrospect ' is remarkable in many ways—not least in its declaration, by one of the three or four really great prehistorians the 20th century has yet seen, of what he was trying to achieve and what he thought he had achieved; it should, incidentally, be read in conjunction with his *History* (Cobbett Press, 1947) and his *Society and Knowledge* (1956).

It was in *Society and Knowledge* that Childe wrote, only three years ago: ' I am an archaeologist and devote my time to trying to gather information about the behaviour of men long since dead. I like doing this and my society pays me quite well for doing it. Yet neither I nor society can see any practical applications for the information I gather; we

are indeed quite sure that it will not increase the production of bombs or butter.' And those who have read his article in ANTIQUITY (1933, 410) entitled ' Is Prehistory Practical ? ' will have seen how difficult he found it to answer the question in the affirmative. In ' Retrospect ' we see him unconcerned with the practical utility of prehistory; here he writes as a scholar looking back on a long and brilliant life of scholarship, and with the modest effacement which was so characteristic of him, misses his own greatness—his great contribution to systems and to absolute dating in chronology, his pan-European background, his synthetic distillation of European scholarship at its best to all of us at our language-bound worst. We once asked Dr Bernabò Brea why he dedicated his *Sicily before the Greeks* to Childe, and he replied: ' Because I did not begin to understand European prehistory until I had read *The Dawn* and *The Danube.*' How true this was for so many people, who learnt, in the twenties and thirties, when they learnt post-Palaeolithic prehistory, Childe's prehistory.

The great puzzle of Childe at all times was to what extent he was a Marxist (or a Marrist) and to what extent he paid lip-service to an Outsider philosophy. He used to complain in public at breakfast in hotels that there was no *Daily Worker* available for him to read, although *The Times* had been delivered to his bedroom with his morning tea. To what extent was his avowed love for Russia and his intellectual contortions in *Scotland before the Scots* a pose ? He has himself described his post-Oxford job in Australia as ' a sentimental excursion into Australian politics '; his Marxism/Marrism was also a sentimental excursion into the use of new archaeological and historical models. It was, most certainly, no conscious pose, for as ' Retrospect ' so clearly shows, Gordon Childe was always deliberately seeking the answer to historical problems.

For a while he sought them in the works of Russian prehistorians and we remember vividly the delight with which he would quote the passage from Mongait's ' The Crisis in Bourgeois Archaeology ' which has now been made available to all of us in Mikhail Miller's *Archaeology in the U.S.S.R.* (Atlantic Press, London, 1956). ' Bourgeois archaeology, like history, is distinguished by extreme idealism (A. Goldenweiser). The English historian Collingwood goes even further . . . and Daniel follows after Taylor. . . . Contemporary bourgeois archaeologists serve the political aims of their governments. . . . More and more often even the scholars who are hostile to us are obliged to turn to the achievements of Soviet Scholarship. . . . Among bourgeois scholars there are not only our ideological enemies, there are also progressive scholars who are friends of our country and who understand very well the universal significance of our science. One of these persons is . . . Gordon Childe. Childe has not yet succeeded in overcoming many of the errors of bourgeois science. But he understands that scientific truth is in the socialist camp and is not ashamed to call himself a pupil of Soviet archaeologists.'

Of one thing we can be sure: Childe's attitude to Soviet archaeological scholarship changed during his lifetime. He had been asked to write a volume on *Russia* for the series of books entitled *Ancient Peoples and Places*, and wrote in the last few days of August, 1957, to the editor of that series: ' I gather you're still hoping to get a book on Russian prehistory out of me. But you won't. . . . Even if one did explore the unpublished collections in remote museum magazines . . . I shouldn't find the evidence to produce a coherent story that would convince me, for I don't believe it yet exists. One cannot just enumerate a number of archaeological " facts " in any old order; they must be set at least in a chronological frame. But the relative and absolute chronology for the neo- and palaeometallic stages is just hopelessly vague. The official Russian schemes are really guesses that do not even attract, still less convince me. Passek's division of Tripolye is terribly subjective and I see no reason why Triploye A (whatever it is) should be put nearer 3000 than 2000 B.C. An equal uncertainty affects the absolute dating of the Kuban culture. . . . I don't feel

inclined to choose between such divergent guesses without convincing evidence. And I think to publish one guess, however often its guesswork character be repeated, is positively harmful and misleading. Yet the choice must be made in arranging the material for a book and the arrangement is thus the assertion of one hypothesis that no amount of reservations will banish from the reader's mind. . . . Until there be evidence to support one well-grounded hypothesis on the main issues a book on prehistoric Russia would be premature and misleading. We may expect C 14 dates to resolve the major issues. But a summary of the Russians' guesses, as though they were facts, *à la Hančar*, is worse than useless.'

We publish these words (which should be read in conjunction with Dr Klindt-Jensen's review of Hančar in the last issue of ANTIQUITY) not only because they are intrinsically interesting, but because they are written by a man who wrote *Scotland before the Scots*. The great value of ' Retrospect ' is that it shows the mental adventure of one of the most distinguished and learned of pre-historians, and shows how he ventured sentimentally and seriously into Marxism and ventured away again. The comments which he wrote in the last few years of his life (and which we have quoted) on Russian prehistory are devastating. We hope they will be answered, and our columns, no less than those of *The New Statesman and Nation*, are wide open to Russian comment and criticism. We have written to the Cultural Counsellors of the Russian and Chinese Embassies requesting them to keep us informed of developments in the knowledge of the ancient past made in that enormous zone of the ancient world from the Volga to Vladivostock, and we welcome the publication in German of Professor Brjussow's *Geschichte der Neolithischen Stamme in europaischen teil der U.S.S.R.* (Akademie Verlag, Berlin, 1957) as much as we eagerly await fuller news of Professor Ying Ta's discovery in Chengchow of a settlement of the Shang-yin dynasty covering 27 square miles, estimated to be larger and earlier than Anyang. There are no frontiers in prehistory, and we hope there will be many prehistorians from eastern European countries and from Russia and China at the Hamburg conference this autumn. We hope too that the widespread rumour that the Russians intend to invite the 1962 International Conference of Prehistoric and Protohistorical Sciences to meet in Moscow, is well-founded. What could be more exciting than a visit to the archaeological treasures of Leningrad and Moscow and Kiev? We welcome the splendid evidence of Russian-British archaeological co-operation for the forthcoming Byzantine Exhibition mentioned below (p. 119).

These words are being written in the eastern marches of Finland and the view from our window over the famous Imatra rapids shows four factories—two in Finland and two in Russia. It is even more ridiculous to draw these hair-line frontiers in archaeology than it is to do so in the modern world—because the modern world demands political entities and affiliations, and in the study of the prehistoric past we have no affiliations. The prehistoric past is illiterate, anonymous and unpolitical. We are all, whether we be in Leningrad or Peking or Harvard or Oxford, studying the same problems, and the nature of our results is an index not only of our knowledge but of our objectivity. It is to the enduring fame of Gordon Childe that his work and his worth were uncurtained, and that the whole world has been the wiser for his scholarship (as for his scholarly adventures) as it is the sadder for his death. Here was a truly great man who did not know how great was his scholarship and his influence, and who was not fully aware from time to time of the ideological camps into which his scholarship was leading him. We mourn his passing and hope that his greatest monument to scholarship and to the revolution in European prehistory associated with his work, namely *The Dawn of European Prehistory*, will be, from time to time, brought up to date and kept in print. But we realize that as scholarship intensifies, there must be very few who will have both the broad vision and the detailed knowledge which Gordon Childe combined in such an excellent, endearing and authoritative way.

We have become accustomed in the last few months to claim and counterclaim about linguistic identifications. The identification of Linear B as Greek, by Ventris and Chadwick (ANTIQUITY XXXI, 1957, 4), has been denounced by Professor Beattie. In our present issue we publish an article from the pen of Professor Pope denouncing the indentification by Professor Cyrus Gordon of Linear A as Akkadian. We believe that the minutiae of linguistic controversy are as much above the head of the average reader of ANTIQUITY as they are above the Editor's head, and that detailed linguistic controversy must be pursued elsewhere, in learned journals reserved for this specialist purpose. What we want here in ANTIQUITY are impartial summaries of controversial and technical disputes written in such a way as we can readily understand their relevance to the facts of ancient history and archaeology, which we, in this journal, seek to review.

Having said that, readers may wonder why we have published the short article from Joyce Reynolds, Olwen Brogan and David Smith on *Inscriptions in the Libyan Alphabet from Ghirza in Tripolitania* that appears in the present number. Is this not, readers may say, a deliberate invitation to detailed linguistic discussion? Not at all: it is publication —and a first publication, we are glad to state—of new and intriguing material. It will be published in full elsewhere. Meanwhile: what do these inscriptions mean, and in what language are they? Address your answers to Mrs Olwen Brogan, 5, Belvoir Terrace, Cambridge, England.

It is rare for matters of archaeological politics to become matters of national discussion. True, in England in the eighteen-seventies, members of the House of Commons had the privilege of hearing Sir John Lubbock and Mr Gladstone on ancient monuments, prehistory and Schliemann's discoveries at Mycenae and Troy. But surely nothing has ever happened at Westminster to compare with the recent discussion of Stonehenge and Hadrian's Wall. On 25 February the Minister of Works answered questions on the reconstruction of Stonehenge and the Wall, and in the course of his speech said what *The Observer* rightly singled out as one of the sayings of the week, namely, that ' there are often two opinions on every subject—the correct and the incorrect '. The discussion was prompted by the announcement of the proposed repair of Stonehenge and by an article by Jacquetta Hawkes in *The Observer* on 9 February in which she accused the Ministry of Works of trying to take ' the spirit of Subtopia ' to the Wall. The Minister of Works agreed that Dame Irene Ward was entirely correct in her assumption that Jacquetta Hawkes's article was ' absolutely inaccurate and unfounded '; but the Minister's ears must have reddened when he found in *The Observer* for 30 March these charges repeated in an article amusingly called ' Back to the Wall '. Mr Francis Noel Baker raised the whole issue on the adjournment of the house at 8.14 p.m. on 2 April in an elegant and learned speech, which initiated a debate that went on until the House adjourned at four minutes to ten. One hour and forty-two minutes of debate on archaeology!—can you believe it, although the date was April the second and not the first? And a debate full of remarkably well-informed and sensible speeches. Of course the main issue has not resolved—indeed the whole problem of repair and conservation at Stonehenge, and at Hadrian's Wall, as at Mycenae, and the Agora and acropolis at Athens, is not easily to be resolved. We shall revert to this general issue again. Suffice it to say here three things: how splendid that the House of Commons could talk archaeology for a hundred minutes—and talk it so well; secondly, how nice to read such praise for the Ministry and the Ancient Monuments Board (but what is happening to Nympsfield, Notgrove and Barclodiad y Gawres—monuments with no M.P.s to defend them?); and thirdly, let us urge every archaeologist to buy *Hansard* 585, No. 86—the thirty-three column adjournment debate on Hadrian's Wall is a collector's piece and compulsory reading.

Retrospect

by the late V. GORDON CHILDE

Professor Gordon Childe died in Australia in October 1957 *at the age of* 65. *A few weeks before his sudden death he had sent to Professor Grahame Clark the article which we print here. He describes it as an autobiographical note, and in it he looks back at his career as a prehistoric archaeologist, describes the influences that affected him from time to time, and assesses what he considers to have been his own achievement in scholarship. It is, as readers of* ANTIQUITY *will see from Professor Piggott's remarks in the following article, and from our Editorial, a characteristically modest but amazing underestimate of the real worth of this great prehistorian whose learning and whose views have so much affected prehistory and prehistorians since he first began writing in* 1915. *Childe here naturally refers to some of his writings; for a full bibliography prepared by Dr Isobel Smith see* Proceedings of the Prehistoric Society, XXI, 1956, 295.

THE most original and useful contributions that I may have made to prehistory are certainly not novel data rescued by brilliant excavation from the soil or by patient research from dusty museum cases, nor yet well founded chronological schemes nor freshly defined cultures, but rather interpretative concepts and methods of explanation. So it is the genesis and development of these to which this autobiographical note is devoted.

Like Gustav Kossinna I came to prehistory from comparative philology; I began the study of European archaeology in the hope of finding the cradle of the Indo-Europeans and of identifying their primitive culture. Reading my Homer and my Veda with the guidance of Schrader and Jevons, Zimmer and Wilamowitz-Moellendorf I was thrilled by the discoveries of Evans in Prehellenic Crete and of Wace and Thompson in Prehistoric Thessaly. Indeed I hoped to find archaeological links between the latter area and some tract north of the Balkans whence similar links might lead also to Iran and India. This search—naturally fruitless—was the theme of my B.Litt. thesis at Oxford and set me trying to discover what I could in the libraries of Oxford and London about the already celebrated ' Pre-Mycenaean ' pottery of the Ukraine and hence of its analogues in the Balkans, Transylvania and Central Europe; for at that time cultures tended to be identified with, not only by, pottery! My Oxford training was in the Classical tradition to which bronzes, terracottas and pottery (at least if painted) were respectable while stone and bone tools were banausic. Starting again from this point in 1922—after a sentimental excursion into Australian politics—I got the idea of a chronological framework at least for central Europe based on the sequence of pottery styles established stratigraphically by Palliardi in southern Moravia on the analogy of Evans's Nine Minoan Periods and the four Thessalian periods of Wace and Thompson; it provided the skeleton of four Danubian periods adumbrated in ' When did the Beaker Folk arrive ? ' (*Archaeologia*, LXXIV, 1925, 159) and the first edition of *The Dawn of European Civilization* (1925), and expanded later to six in *The Danube in Prehistory* (1929). And these relative ceramic periods were to be given absolute values by influences from the Aegean, again reflected mainly in pottery.

At the same time my original quest lured me on northwards to Jutland and eventually

to Scotland—rumours of the Abercromby Chair accelerating my progress in the latter direction! In so doing I accumulated a volume of facts and a number of notions that were unfamiliar to English prehistorians; for these came either from the Classical school or were disciples of de Mortillet, wedded to the typology of flints ; the influence of Abercromby and Bryce was virtually confined to Scotland. From Continental literature I absorbed the German concept of a culture, defined but not constituted by distinctive pottery and representing a people. (Abercromby and Petrie had both grasped the notion, but never got it across to the official keepers of British Archaeology.) I came across Schliz's article, ' Die schnurkeramische Kultur ', and subsequently its elaborations by Wahle and Reinerth; at that time the significance of the loess for Neolithic settlement and of forest as a barrier thereto were strangely unfamiliar in Britain though Myres had long been preaching a geographical approach. Eventually I stumbled upon Gams and Nordhagen and so introduced the idea of post-glacial climatic changes to English readers who had of course ignored Geikie. So to the British public many of the ideas presented in *The Dawn* in 1925 were as novel as the various Continental cultures disconcertingly thrust on to the map of a supposedly uniform Neolithic Europe—indeed on to four superimposed maps.

Still *The Dawn* aimed at distilling from archaeological remains a preliterate substitute for the conventional politico-military history with cultures, instead of statesmen, as actors, and migrations in place of battles. The sole unifying theme was the irradiation of European barbarism by Oriental civilization, a traditional dogma in Britain my faith in which had been consolidated as much by a reaction against the doctrines of Kossinna and Hubert Schmidt (which were recognized as slogans of Germanic imperialism more easily after the victory of World War I than they are after the disillusionment of World War II) as by the Diffusionist thesis of Elliot Smith and Perry which at the time was arousing quite sectarian passions among anthropologists and prehistorians. Yet the sea-voyagers who diffused culture to Britain and Denmark in the first chapters in the first *Dawn* (1925)—and they were relegated to a secondary position in the second edition of *The Dawn* (1927)—though they do not hail from Egypt, yet wear recognizably the emblems of the Children of the Sun.

On the other hand, the principal channel of diffusion was already the Danube thoroughfare though, inspired by J. L. Myres and behind him Schrader and Jevons, I looked with over-credulous eyes for footprints of Steppe horsemen in the marshes of the Pripet. But the footprints of migrants and missionaries alike remained isolated culture traits torn from their contexts while typological equivalence even in pottery was accepted as a chronological horizon of continental dimensions. This was childish, not Childeish. Yet, after all, the Three Ages had been explicitly abandoned as a chronological framework; they did not, like Pleistocene and Holocene, label divisions of sidereal time, as many English and German prehistorians seemed still to think, but chapter headings in local archaeological records. Distinctively Childeish was the endeavour to make these labels informative by adopting food-production as the differentia of the Neolithic. The expression at least is derived from Elliot Smith, but its selection was an attempt to endow—or re-endow; for originally the beginning of food-production had been supposed to coincide with that of stone polishing—a convenient typological division of the archaeological record with a human, or at least economic, significance. The next step was taken in *The Bronze Age* (1930).

By that time I had elaborated my chronological scheme in *The Danube in Prehistory* and become Professor at Edinburgh. As such I felt obliged to lecture on the Old Stone and Iron Ages and to give my students at least a survey of the beginnings of food-production, the rise of civilization and—on the assumption I accepted—the bases of the absolute chronology of European prehistory. I had to apply the culture concept to an age when

cultures were accepted as defining periods of global time (Menghin and Rellini were doing this already, but it was five years or more before Aurignacian was contrasted with a partly contemporary Perigordian and Gravettian). And I had also to assemble the data for Mesopotamian and Indian prehistory; for the first scientific excavations in those provinces were just starting and their results known only from brief reports in the *Antiquaries Journal*, the *Journal of the Royal Asiatic Society*, or the *Illustrated London News*. This necessity was the pretext for *The Most Ancient East: the Oriental prelude to European prehistory* (1928). Its compilation contributed to the writing of *The Bronze Age* and familiarized me with archaeological data which can be illumined from written texts as well as, or instead of, just illustrating the latter.

In *The Bronze Age*, each chapter heading in the archaeological record likewise became informative. If the criterion for inclusion be the *regular* use of bronze or even copper for the principal cutting tools and weapons, that implies *regular* trade and social division of labour. I had not yet read Spengler but I assumed that metal was the first *indispensable* article of commerce (as contrasted with luxuries which, at a pinch, society could do without and had admittedly been traded even in the Old Stone Age) and that metal workers were always professionals, who did not grow their own food. (I had not yet realized the distinction, so happily expounded by C. S. Coon, between part-time and full-time specialists.) The former assumption is necessarily *a priori* and is not contradicted even by the Melanesians' *kula* traffic; the professionalism of smiths I inferred from ethnographic evidence, but the status of metal workers in a Sumerian temple-city and an Indian village community is not too easy to reconcile with this assumption. Still the regularization of—intertribal or international—trade gives an economic significance to the chapter-heading even if its sociological implication be not quite so certain.

By this redefinition of the Bronze Age I was finally committed to an economic interpretation of archaeological data, and I drew the appropriate corollaries. Obliged by unexpected new discoveries to rewrite *The Most Ancient East*, I not only read excavation reports but visited Mesopotamia and India. I saw how the beginnings of literacy in the three great river valleys coincided with the erection of the first monumental tombs and temples and the aggregation of population in regular cities. Indeed at Ur and Erech I saw how rustic villages had grown into vast townships just as English villages had grown into manufacturing towns. Now the latter transfiguration was familiarly attributed to an ' industrial revolution '. Demographically the birth of literacy in the Ancient East also corresponded to a revolution, the Urban Revolution. The upward kink in the population graph, deduced from the monuments, must be due at least partly to the emergence in addition to the farmers of a new order of professionals who did not grow or catch their own food. (The epithet ' urban ' may have unduly exaggerated the numerical importance of this order; of course primary producers enormously preponderated throughout the ancient world, and in Egypt it is not proven that professionals were aggregated in cities or towns under the Old Kingdom though Frankfort's arguments to the contrary are quite inconclusive.) But if the Urban Revolution had added an order of professionals to the farmers, the latter were themselves the offspring of a revolution. The adoption of food-production must have been, and, from the available data, had been, followed by a still greater expansion of population than on the foregoing analogy would amply justify the term ' Neolithic Revolution '. So in *New Light on the Most Ancient East* (1934), despite the occasional invocation of undocumented events in the wings, a truly historical pageant of economic development was presented on the stage.

In the same year I visited for the first time the U.S.S.R. and secured some typical Russian works on prehistory. From Kruglov and Podgayetsky, Krichevskiĭ and Tretyakov

I learned how neatly even the Marrist perversion of Marxism explained without appeal to undocumented external factors the development of certain prehistoric cultures in the Union. So I at least took over the Marxist terms, actually borrowed from L. H. Morgan, ' savagery ', ' barbarism ' and ' civilization ' and applied them to the archaeological ages or stages separated by my two revolutions: Palaeolithic and Mesolithic can be identified with savagery; all Neolithic is barbarian; the Bronze Age coincides with civilization, but only in the Ancient East.

About the same time I had been asked to write the prehistoric and early Oriental chapter in a projected Marxist History of Science. Though I did not attempt to master hieroglyphic or cuneiform I read a number of articles in *Isis* and some books on ancient science, above all Neugebauer, and that just when pictographic account tablets from Erech and Jemdet Nasr were figuring in excavation reports. Moreover, I adopted the quite respectable view that the lore, successfully applied by craftsmen, has contributed quite as much to modern Natural Science as the rather futile speculations of literate astrologers, hepatoscopes and alchemists. So craft processes and products that archaeologists can observe can stand beside mathematical tablets and surgical papyri as genuine documents in the history of science which no longer begins with the invention of writing. (For this attitude I earned the special commendation of Sarton, its editor, in *Isis*.)

Since ' means of production ' figure so conspicuously in the archaeological record, I suppose most prehistorians are inclined to be so far Marxists as to wish to assign them a determining role among the behaviour patterns that have fossilized. They can do so even in the U.S.A. without invoking the 5th Amendment, since it was to the ' mode of production ' (' means ' plus ' relations ') that Marx attributed such a dominating influence. By 1936 I had advanced beyond this in so far as I insisted on the need for concentrating the surplus to accomplish the Urban Revolution and so recognized the Hegelian rationality of the political and religious totalitarianism that characterized the ancient Oriental States. (The term ' totalitarian ' I adopted later from Heichelheim while its applicability to the Indus civilization was only demonstrated by Wheeler's excavations after 1945.)

With the aid of the foregoing concepts in *Man makes Himself* (1936) the archaeological record is interpreted as documenting a directional process wherein men by applications of science steadily increased their control over non-human nature so that their species might multiply and incidentally secrete laws and political institutions, religions and art. That falls short of Marxism in so far as it failed to emphasize that and how science can only be applied, means of production only operated, within an institutional framework that is not itself entirely economic. Still it is genuine history extracted mainly from unwritten documents.

In the new *Dawn* of 1939 I payed lip-service to ' Marxism ' in a standardized scheme for the description of cultures: first the food quest, then secondary industries and trade, only thereafter social and religious institutions in so far as they can be inferred or deduced. Though the cultures, thus described, were supposed to stand for societies, they did not develop autonomously; the events remained external as before, movements of peoples, and the unifying theme was still irradiation by Oriental and Aegean civilization so that nothing distinctively European emerged. (Embittered hostility to and fear of the archaeological buttresses of Hitlerism enhanced my reluctance to recognize the positive aspects of all European barbarisms.) I took from Marxism the idea of the economy as the integrating force in society, but I was just as much influenced by Malinowski's functionalism and tried to stick the archaeological bits together by reference to their possible role in a working organism. Still there are passages which, despite extreme compression, hint at truly historical conjunctures of environmental change, internal economic progress and external

stimuli. Such historical explanation found more scope later when Iversen's *Landnam*, and Grahame Clark's ' Farmers and Forests in Neolithic Europe ' (ANTIQUITY, 1945, 57) had cleared the wooded barriers to settlement and revealed even prehistoric man as an agent in environmental change.

But this release from geographical determinism came only after *What happened in History* (1942) which was a real contribution to archaeology as a concrete and readable demonstration designed for the bookstall public that history as generally understood can be extracted from archaeological data. I wrote it to convince myself that a Dark Age was not a bottomless cleft in which all traditions of culture were finally engulfed. (I was convinced at the time that European Civilization—Capitalist and Stalinist alike—was irrevocably heading for a Dark Age.) So I wrote with more passion and consequently more pretentions to literary style than in my other works. It presented a smoothly continuous historical narrative based equally on archaeological and textual data. It bears witness to the truth that no chasm, not an unconformity even, divides prehistory from history, and so validates archaeology's claim to provide unwritten historical documents as informative and reliable as written texts. And it gave its testimony before a public far wider than a couple of thousand readers interested in the past; over 300,000 copies have been sold. Still the conceptual framework and explicative mechanisms have advanced but little since *Man makes Himself* and discredited fictions like ' Economic Man ' still haunt its pages.

It was only later that I reread Soviet prehistorians in the brief interval during which a sympathetic attitude to the U.S.S.R. was not only useful for getting information on an archaeological province that was crucial for my own special interests, but was also commended by public opinion and the State. In this atmosphere I came to appreciate better the value of even the perversion of Marxism, subsequently branded as Marrism. Its principles I applied in *Scotland before the Scots* (1946). The cautious application of working hypotheses that were frankly stated there did give a picture of Scotland's development which was far more realistic and far more historical than had been achieved with migrationist hypotheses in *The Prehistory of Scotland* (1935). Yet I just had to admit migrations and the impact of foreign cultures: the internal development of Scottish society in accordance with ' universal laws ' simply could not explain the archaeological data from Scotland; reference to Continental data actually documented the solvent effects of external factors.

My incredibly bad chapter in *The European Inheritance* (edited Barker, Clark and Vaucher, 1954) was dominated by an old-fashioned over-estimation of the Orient's role, and utterly missed the individuality of prehistoric Europe, while in *Prehistoric Migrations in Europe* (1950) a return to my original quest for the Indo-Europeans completely failed to locate the cradle, while its plausible identification of the Indo-Europeans in Europe with Urnfield-folk was refuted within ten years by the Ventris-Chadwick decipherment of the Mycenaean script and the discovery of earlier wheeled vehicles north of the Alps.

In pursuit of an early interest in philosophy, which since 1913 has fascinated me as much and as unprofitably as the Indo-European Urheimat, I was led to a sociological approach to epistemology and the discovery of Durkheim and a deeper appreciation of his master, Marx. Now at last I rid my mind of transcendental laws determining history and mechanical causes, whether economic or environmental, automatically shaping its course. Incidentally I realized that the environment that affected a prehistoric society was not that reconstructed by geologists and palaeobotanists but that known or knowable by the society with its then existing material and conceptual equipment. (Neither arable land nor ores figure in the historical environment of a Palaeolithic horde.) A society's scientific knowledge in turn is limited by its economic and social organization. (It would be misleading

to attribute a knowledge of bronze-casting to a society unless an adequate supply of metal was guaranteed, for neither memories fading into myths nor the secrets of a single family deserve the adjective scientific.) This completed my equipment for venturing upon a prehistory of Europe that should be both historical and scientific.

In rewriting *New Light on the Most Ancient East* in 1954 and *The Dawn of European Civilization* in 1956, I began to recognize how right Hawkes had been in 1940 when in his *The Prehistoric Foundations of Europe* he had insisted that, by the Bronze Age, Europe had achieved a kind of culture distinctively its own. I saw not only that this was so but also why. I invoke no agencies external to the observed data, no eternal laws transcending the process as empirically given, but historical conjectures of well-established environmental circumstances and equally well-known patterns of human behaviour legitimately inferred from their archaeological results. The archaeological data are interpreted as the fossilized remnants of behaviour patterns repeatedly illustrated in ethnography and written records. Together with the relevant features of the non-human environment they are presented as instances of more general known processes. So the specific events are explained as individual and perhaps unique conjunctures of known universal factors. Such explanation is scientific as well as historical. It is just so that a geologist would explain the peculiarities of a particular site, where a dam is to be built or a mine shaft sunk, as the resultant of general characters of the minerals exemplified there and the equally general processes of folding, faulting, erosion . . . to which they have been exposed at this precise spot. For natural, like human, history explains particulars by describing them as exemplifying universal laws, and these laws are no longer regarded as imposed upon nature from outside, but as generalized descriptions of what has been observed inside; so ' laws of nature ' are not necessary but very highly probable. At the same time, as soon as an event is seen to conform to such a law, it is recognized as rational and logically necessary, or, in a word, understood. Admittedly laws of human behaviour are so far less highly probable than the laws of chemistry and physics that the term law is deceptive. Yet they are or would be of the same kind.

Now I confess that my whole account may prove to be erroneous; my formulae may be inadequate; my interpretations are perhaps ill-founded; my chronological framework—and without such one cannot speak of conjunctures—is frankly shaky. Yet I submit the result was worth publishing.[1] It is a final answer to those who told us: ' the true prelude to European history was written in Egypt, Mesopotamia and Palestine while the natives of Europe remained illiterate barbarians '. It exemplifies better than any other work I know how what everyone will accept as history could be extracted from archaeological finds; whether the particular extract be accepted or no, it should help to confirm the status of archaeology among historical disciplines. At the same time it illustrates what scientific history ought in my opinion to be like. Incidentally it emphasizes once more the urgency of establishing a reliable chronology; a great deal of the argument depends on a precise date for the beginning of Unětice that is at best very slightly the most probable guess out of perfectly possible guesses ranging over five centuries.

[1] Professor Childe is referring here to his last book, *The Prehistory of European Society*, discussed by Professor Piggott in the following pages. Ed.

The Dawn: and an Epilogue

by STUART PIGGOTT

Professor Piggott here reviews the sixth edition of The Dawn of European Civilization *published a few weeks before the death of Professor Gordon Childe* (*pp. xiii* + 368, 159 *figs*, 4 *maps*, *Routledge and Kegan Paul*, 1957, 42*s.*) *and* The Prehistory of European Society (*pp.* 183, 2 *maps*, *Penguin Books*, 1958, 3*s.* 6*d.*)

A YEAR before the first number of ANTIQUITY appeared, its founder reviewed a new publication for the *Antiquaries Journal*.[1] ' This book,' he began, ' is the most important of its kind that has hitherto been published. . . . Never before has the whole field of European origins been surveyed by a specialist who can also generalize, who writes clearly and intelligibly, and who is apparently familiar with all European languages. Its publication brings us appreciably nearer to the ultimate goal of our study.'

Crawford saw that Gordon Childe's *The Dawn of European Civilization*, in its first edition of 1925, marked the beginning of a new era in archaeological studies in this country. I suspect that he was one of the very few British prehistorians at that time who realized the fact and appreciated the implications. More than thirty years later a revised sixth edition has been published, appearing almost simultaneously with the tragic news of the author's death in his native Australia. For a full generation *The Dawn*, as we have all affectionately called it, has been with us, the indispensable reference-book for student, teacher and interested layman, our Ariadne-clue in the labyrinth of Neolithic and earlier Bronze Age Europe. It is worth while making the attempt, not simply to estimate the extent to which the last edition modifies its predecessors, but to consider Childe's contribution to archaeology at large, made through this his most famous work, and to glance at the impact of the original book on archaeological thought in the 1920's.

The study of prehistory in this country at that time was of course largely an amateur pursuit, with the exception of scholars such as those in the British Museum and at Cambridge, and Evans and Myres at Oxford. But I think *The Dawn* was not the only portent of a change to what we can now recognize as a modern mode of thought, for in 1923 Fox had published his *Archaeology of the Cambridge Region*, as revolutionary a work in its own field as was Childe's in the much more complex European landscape. Apart from this, however, we must admit that, at its best, most of British prehistory at that time was sadly provincial. At the beginning of the century, British archaeologists had in fact grasped the relationships in prehistory of these islands to the European continent: Abercromby is the name we immediately think of in this context. But we must not forget the others—in Scotland Bryce, and Robert Munro, who so charmingly announced that, on being invited to give the Rhind Lectures to the Society of Antiquaries of Scotland on lake-dwellings, ' my first and almost immediate step was a hasty run to the principal centres of lake-dwelling researches in Europe ', soon followed by further visits in which ' my wife and I perambulated the whole of Central Europe with note and sketch books in hand '.[2] This was in fact in 1888; in England Sir John and Arthur Evans were similarly looking at British

[1] *Antiq. Journ.*, VI (1926), 89–90.
[2] R. Munro, *The Lake-Dwellings of Europe* (1890), v, vii.

prehistory from a European view-point, and the latter's paper on the Aylesford cemetery published in 1890 [3] is still a definitive work in the study of Continental and British relationships in the Early Iron Age. On the eve of the First World War, Bushe-Fox was percipiently looking across the Channel for further Iron Age contacts.[4]

But after that war, view-points were narrowed down to insular dimensions. Perhaps a certain chauvinism supervened, a desire to turn one's back on the Continent, and in particular on Germany, and to potter about in a quiet back garden. Almost alone in England, the European view-point in prehistory seems to have been preserved in such figures as Crawford and Harold Peake, and in the enigmatic figure of Reginald Smith in the British Museum, reading all the foreign periodicals as they came into the Department of British and Medieval Antiquities, and as often as not distilling his wide comparative knowledge of prehistory and early medieval times in anonymous paragraphs in the newly founded *Antiquaries Journal*, in a style immediately recognizable to the connoisseur by its sometimes Delphic quality of obliquity. Scottish prehistory had entered its most provincial phase, scarcely permitting itself even a southwards glimpse across the Border.

In this climate of thought *The Dawn* was not easy to accept. In the *apologia pro vita sua* published in this number of ANTIQUITY (pp. 69, ff) Childe points out some of the ways in which his ideas were completely novel to the British archaeologists of 1925—the concept of an archaeological culture, the distinction of such cultures within what was a supposedly uniform Neolithic Europe, the relevance of the post-glacial climatic changes. But there were other novelties too. The book introduced the concept of European prehistory seen as a whole, and indeed in its relationship to the Ancient Orient, with the British Isles appearing only in a final chapter the title of which clearly demonstrated their position in the general picture—' Atlantic and Central European Cultures in Britain '. The writing of *The Dawn* was the first step in the establishment of a tradition of European prehistoric studies in this country, a tradition developed by Childe in his subsequent published work and in his formal teaching and informal contacts with the younger generation. Once he had shown the way, there was no longer any excuse for an insular view-point.

The sound and perceptive planning of the book from the start is evidenced by the manner in which it has substantially retained the same framework throughout all its editions. The basic thesis of the work was announced in the preface to the first edition—' The Occident was, I submit, indebted to the Orient for the rudiments of the arts and crafts that initiated man's emancipation from bondage to his environment and for the foundation of those spiritual ties that co-ordinate human endeavours.' The book was therefore planned in such a manner as to demonstrate this postulate by the evidence of the archaeological material, however much we who use *The Dawn* may forget this fact, and regard the book only as a factual summary of the content of the main cultures in Europe during the late third and earlier 2nd millennium B.C. Of course in a sense it is this, too, but as ' Retrospect ' tells us, Childe's life work followed a consistent course in pursuit of an aim again expressed in the preface of the first edition of *The Dawn*. ' My theme is,' he wrote, ' the foundation of European Civilization as a peculiar and individual manifestation of the human spirit. . . . Among the Early Bronze Age peoples of the Aegean, the Danube valley, Scandinavia and Britain, we can recognize already the expression of those very qualities of energy, independence, and inventiveness which distinguished the western world from Egypt, India or China.' In the last edition a new, short, chapter was added; four and a half pages entitled ' Retrospect: The Prehistory of European Society '. Within a few months there has been posthumously published his Pelican Book of the same title.

[3] A. J. Evans, ' On a Late-Celtic Urn-field at Aylesford, Kent ', *Arch.*, LII (1890), 1–74.

[4] In his report on *Excavations at Hengistbury Head* (1915).

In the 1930's one of us wrote a Ballade to Childe, using as its refrain his words, often and rightly repeated when asked for some recondite reference—' You'll find it in a footnote in *The Dawn* '. The envoi ran:

> ' Prince! To your learning be all honour due!
> How will eternity treat man, its pawn?
> Leaving the past do you then turn and view
> Man's end as but a footnote to his Dawn? '

In *The Prehistory of European Society* he did, in effect, give his answer. ' The central object of this book,' he wrote, ' is to show that even in prehistoric times barbarian societies behaved in a distinctively European way.' He had returned to the theme set out in the first edition of *The Dawn*, the unique quality of European civilization already perceptible in prehistory, the peculiar circumstances which could lead one to regard ' the metics at Athens, the wayfaring journeyman of the Middle Ages, and the migrant craft unionist of the 19th century ' as ' the lineal descendants ' of the craftsmen in metal of earlier Bronze Age Europe.

The book is planned as a complementary volume to *The Dawn*, where, as he characteristically wrote, the basic documentation will be found ' buried under a forbidding accumulation of outlandish culture-names and references to obscure periodicals '. A lifetime's study of this material had led Childe to perceive in barbarian Europe the emergence of the metal technologist as an independent craftsman, moving about within and between the local tribal units, and freed from the bondage of food-production. Unlike his counterpart in the ancient Oriental civilizations, he was not reduced to economic servitude within the class divisions established as a result of the Urban Revolution: he was a free professional craftsman, able to ' shake off the bonds of allegiance to an overlord or the more rigid fetters of tribal custom '. Bronze Age Europe provided the preconditions for the emergence of a technocracy.

It is an interesting thesis: as we see from ' Retrospect ' Childe had been attracted by Hawkes's recognition of the peculiar quality of Bronze Age Europe in his *The Prehistoric Foundations of Europe*, published in 1940, and had determined to seek the cause, and thought he had found it in the archaeological evidence. In assessing the contribution made to our studies by *The Dawn* and its companion volume (implicit from the first, though only achieved over thirty years later) we must still consider two points. In the first place, does our estimate of Childe's contribution agree with what he himself thought his major achievement, and, in the second, what of certain limitations in his approach?

We now know what his own estimate was: the originator of new ' interpretative concepts and methods of explanation ' rather than the construction of chronological schemes and the definition of archaeological cultures. But surely this is an all too characteristically modest underestimate? To the ' professional ' archaeologist (to use this horrid term), Childe is above all the systematizer, the lineal descendant of Montelius as an acute and unwearied constructor of chronological schemes, the man who could survey the European scene with scholarly detachment and always distinguish the prehistoric woods amid the close-set trees of archaeological detail. Students and teachers, we use *The Dawn* as we use *The Danube in Prehistory*, not as the demonstration of a theme, but as a secondary source, descriptive of European prehistoric cultures to about 1500 B.C. If Childe did not in fact conceive of *The Dawn* in these terms, it becomes easier to understand why in all editions its illustrations were so bad and inadequate. He was unappreciative of the visual presentation of archaeological material in all his works, but if *The Dawn* is to be regarded, not as a source-book for the cultures of Europe in the first half of the 2nd

millennium B.C., but an exposition of the relationship of the cultural traditions of the Orient as against those of Europe, the scrappy and scratchy text-figures become incidental to the thesis, not an essential demonstration of it.

To the archaeologist then, Childe is surely the interpreter of a vast and complex mass of archaeological material in comprehensible form, and one of the most profound students of the problems of European prehistoric chronology within his chosen period. To the general public, on the other hand, he is known as the author of *Man Makes Himself*, *What Happened in History*, and now *The Prehistory of European Society*. In a word, his contribution here would appear to be that which he himself valued the most.

Archaeology is largely a descriptive discipline, but this description is meaningless unless conducted within some framework of reference. Since the existence of archaeological evidence as a potential source of knowledge about the human past was recognized, it, like other approaches to history, has been regarded in terms of what the scientist would call a *model* of the past—an intellectual construct which will enable us to arrange the observed phenomenon in a meaningful pattern involving cause and effect.[5] As in scientific usage, more than one model can exist at a time, each having a partial validity. The earliest models of the past were naturally theological, and teleological; later models include the most famous of them all, the technological-evolutionary model for prehistory based on the changing materials used for edge-tools, embodied in the familiar Three Ages of Stone, Bronze and Iron. A current alternative is an economic model, wherein the means of subsistence is used as a criterion, and food-production is contrasted with food-gathering.

Though he never expressed it in explicit terms, Childe assembled and interpreted his data in terms of these last two models. Both of them, and particularly the technological-evolutionary one, work particularly well with archaeological material, because it is, in its nature, largely the products of material technology. Hawkes made just this point when he described the increasing scale of difficulty in archaeological inference in four stages, beginning with the inferences regarding techniques, then going on to those concerned with the subsistence-economics of the society studied, then to the social-political institutions and finally to religious institutions and spiritual life.[6] The first is comparatively straightforward, the second only slightly less so, but the third and fourth strain the limits of legitimate inference to the utmost, or are outside its scope when one is concerned exclusively with archaeological evidence.

Using a combination of the view-points provided by the technological-evolutionary and the economic models, Childe tried to make inferences involving Hawkes's third stage of difficulty and unreliability (for Childe, the fourth stage would be subsumed in the third), and we can only discuss their likely validity in terms of the nature of archaeological evidence and its limitations. This is an aspect too little considered,[7] but a frank recognition that the nature of our perception of the past obtained by archaeological means is directly conditioned by the peculiar qualities of the evidence involved must be a necessary prerequisite to making any inferences at all. Collingwood and Frankfort were, of course, aware of this problem, and the latter did in fact demonstrate the insufficiency of a purely archaeological model for explaining the divergent forms taken by Egyptian and Mesopotamian civilizations,

[5] I had been using this concept for prehistory in lectures in 1957–58, unaware that Mr R. W. K. Hinton was doing the same in the context of modern history at Cambridge, until his broadcast on this theme at the beginning of this year (printed in *The Listener*, February 6th, 1958, 233). The idea is implicit in Frankfort's claim ' that a viewpoint whence many seemingly unrelated facts are seen to acquire meaning and coherence is likely to represent a historical reality ' (*The Birth of Civilization in the Near East* (1951), 21).

[6] C. F. C. Hawkes, ' Archaeological Theory and Method: Some Suggestions from the Old World ', *Amer. Anthrop.*, LVI (1954), 155–68.

[7] But cf. M. A. Smith, ' The Limitations of Inference in Archaeology ', *Arch. News Letter*, VI (1955), 1–5.

though sharing a common background of technological development.[8] Childe's interpretations of the past will continue to be the subject-matter for discussion, but it seems likely that it is his organized presentation of the material, his perception of the pattern of prehistoric cultures in time and space, his assessment of chronological probabilities, and his constant recognition of the relationship between the civilized and barbarian peoples of the Old World in antiquity which will be his most enduring memorial in the world of scholarship.

[8] Collingwood's discussion of the nature of archaeological evidence is contained especially in *The Idea of History* (1946) and his *Autobiography* (1939); Frankfort's in *The Birth of Civilization in the Near East* (1951), especially Chapter I.

Book Chronicle

We include here books which have been received for review, or books of importance not received for review, of which we have recently been informed. We welcome information about books, particularly in languages other than English and American, of interest to readers of ANTIQUITY. *The listing of a book in this chronicle does not preclude its subsequent review in* ANTIQUITY.

THE ARCHAEOLOGY OF THE ROMAN EMPIRE: A SCHEME OF STUDY. *By* PROFESSOR I. A. RICHMOND. *pp.* 21. *Oxford*, 1957. An inaugural lecture delivered before the University of Oxford on 14 May, 1957.

ETHIOPIAN ITINERARIES *c.* 1400–1524. *Edited by* O. G. S. CRAWFORD. *pp.* xxix + 232, 2 *plates*, 21 *sketch maps. Published for the Hakluyt Society by the Cambridge University Press*, 1958. 30*s.* This is the last publication of the late editor of ANTIQUITY and emerged from his historical study of the Fung Kingdom of Sennar. It contains the Zorzi Itineraries (translated by C. A. Ralegh Radford) which give a fairly detailed account of Ethiopian geography before the country was overrun in the 16th century by the armies of Granh and by the Gallas.

VANISHED CITIES. *By* HERMANN *and* GEORG SCHREIBER. *Translated from the German by Richard and Clara Winston. pp.* 358. *Weidenfeld and Nicolson*, 1958. A good account of lost cities from Babylon, Palmyra and Troy to Baiae.

RIOT IN EPHESUS. *By* CHARLES SELTMAN. *pp.* 172, 1 *plate*, 1 *map. Max Parrish.* 21*s.* Writings on the heritage of Greece: a posthumous book of essays, with a preface by Peter Quennell, and bibliography of Seltman's writings.

HISTORY OF THE BRITISH IRON AND STEEL INDUSTRY from *c.* 450 B.C. to A.D. 1775. *By* H. R. SCHUBERT. *pp.* 445, 36 *figs.*, 26 *plates*, 11 *maps. Routledge and Kegan Paul*, 1957. 60*s.*

THE STONES OF LLANDAFF CATHEDRAL. *By* F. J. NORTH. *pp.* xvii + 122, 10 *figs.*, 21 *plates. University of Wales Press, Cardiff*, 1957. 15*s.*

CHINESE ART. *By* WILLIAM WILLETTS. Vol. I: *pp.* xxxiv + 391. Vol. II: *pp.* vi + 802, 64 *plates*, 110 *figs.*, 6 *tables*, 7 *maps. Penguin Books.* 7*s.* 6*d. each.* A continuous record of 4,000 years of Chinese art history. Vol. I contains first an account of the geographical background and prehistory of Chinese society. Thereafter in this and vol. II eight art-forms are discussed, namely those of jade, bronze, lacquer, silk, sculpture, pottery, painting and calligraphy (treated as one) and architecture. The author's method is descriptive. Copious illustration takes the place of aesthetic judgements. Astonishing value for 15*s.*

[*continued on p.* 143

The Corinthian Isthmus and the Isthmian Sanctuary

by OSCAR BRONEER

Professor Oscar Broneer of the University of Chicago has been in charge of the excavations at Isthmia near Corinth since 1952, *working on behalf of Chicago and the American School of Classical Studies in Athens. Here he describes some of his recent results including the ' enigma ', as he called it in two recent articles in* Archaeology (*June and December,* 1956) *and which he interprets as a complicated device for starting runners on their course.*

THE PELOPONNESUS came so near to being a true island (Island of Pelops) that the neck of land (FIG. 1) which joins it to the northern half of the Greek peninsula is less than five miles wide. This became a natural crossroads of the Greek world. Several routes converging on this landbridge connected the Peloponnesus with the rest of Greece. Communications by sea between the east and the west, through the Saronic and the Corinthian Gulfs, were interrupted by the Isthmus, and attempts were made early to pierce or overleap the barrier. The southern sea-lane encircling the Peloponnesus was much longer, and weather conditions made the journey hazardous. For this reason cargoes were unloaded at the two harbours, Kenkreai on the Saronic and Lechaion on the Corinthian Gulf, and carried by land across the Isthmus. Naval vessels, and perhaps the empty freight carriers, were transported over a paved roadway called Diolkos. The western end of this portage has long been recognized on the Peloponnesian side of the Corinth Canal, and recently longer stretches of pavement have been laid bare on both sides of the Canal[1]. The Diolkos here did not run straight but ascended the steep slope in great sweeping curves (FIG. 1 and PLATE IX (*c*)). The pavement has a width of 3·50–5 m. and is made with large *poros* blocks well fitted together. Two deep ruts, 1·50 m. apart, show that the ships were hauled on wheeled cradles, not on rollers, as was formerly assumed. The excavation, conducted by the Archaeological Service of the Greek Government, is still in progress, and the exact course across the Isthmus will not be known before this work is completed.

As early as the time of Periander the plan of piercing the Isthmus with a canal was conceived; it was revived by Demetrius Poliorcetes, Julius Caesar, Caligula, Nero, and Herodes Atticus. Only Nero, so far as our records show, did anything to implement the scheme. In the year A.D. 67 he broke ground for the canal with a golden pickaxe. At the west end his engineers cut a trench, 2,000 m. long and 40 to 50 m. wide, and at the east end they made a second cutting, 1,500 m. long. In the higher ground between these cuttings they dug test pits, some reaching a depth of over 40 m. It has been estimated that five to six thousand men worked at the canal for about four months and removed some five million cubic metres of earth. Nearly all traces of these operations were obliterated when the present canal was dug between 1882 and 1893.

The Greeks took advantage of the physical barriers in this area to fortify the southern part of the Hellenic peninsula against invasion from the north. The earliest trans-Isthmian wall, discovered by the University of Chicago expedition in the autumn of 1957, was apparently constructed in late Mycenaean times. We have been able to trace its course for a distance of nearly one mile, from the modern village of Isthmia, near the east end of

[1] See article by Nicholas M. Verdelis, *Illustrated London News*, 19 October, 1957, pp. 649–651. Mr Verdelis has kindly furnished the photograph for PLATE IX (*c*).

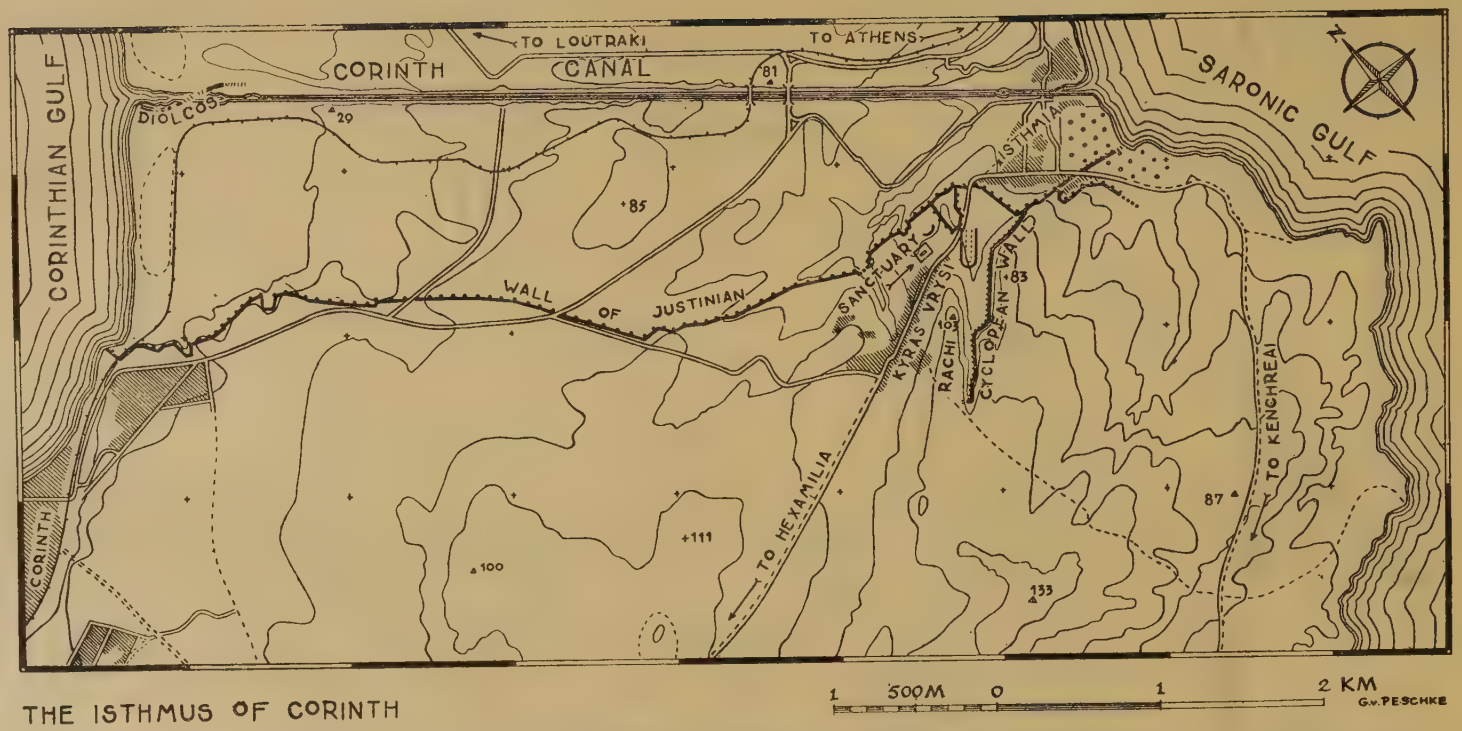


FIG. 1. The neck of land that joins the Pelopponesus to the northern half of the Greek peninsula is less than five miles wide. The Corinth Canal is 6,939 yards long.

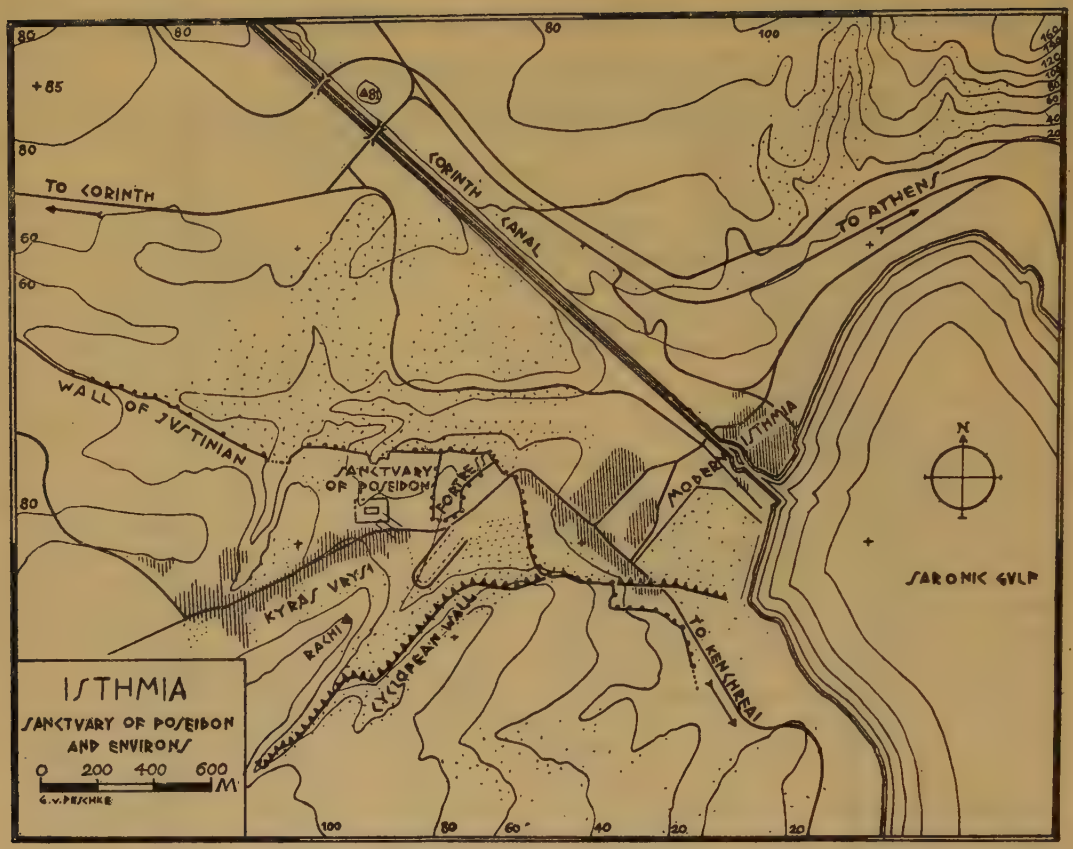


FIG. 2. The Sanctuary of Poseidon and environs.

the Corinth Canal, as far as the Sanctuary of Poseidon (FIGS. 1 and 2). Not all of this stretch is preserved, but enough remains to indicate a uniform and connected line of defence. The masonry is in typical Cyclopean style. Large undressed stones are used for the two faces, and the intervening space is filled with smaller stones and earth. Where both faces are preserved the wall has a thickness of 4–4·60 m. On the north side, i.e. the side from which attacks were expected, there are buttress-like towers, *ca.* 2·50 m. wide and projecting only 0·70 m. from the face of the wall. In the longest unbroken stretch of 43 m. there are four of these towers at intervals of 7·90–9·50 m. At one point the outer face of the wall is preserved to a height of 2·50 m.[2] The position of the towers and the fact that wherever possible the builders took advantage of the terrain to make the approach from the north over steeply rising ground indicate that the wall was built to ward off invasions from the north into the Peloponnesus.

The pottery from the fill of the wall is Late Helladic III B, from the second half of the 13th century B.C. This was the time when the Mycenaean cities in the Peloponnesus and the Greek mainland reared their imposing fortresses, which appear impregnable to the type of military equipment then in use. The wall across the Isthmus adds further evidence for the theory which I have long held,[3] that the Cyclopean walls of Mycenae, Tiryns,

[2] This wall was first noticed by Chrysoula Kardara in 1954 while supervising excavation on the Rachi. I was then under the impression that this piece of Cyclopean masonry was part of a supporting wall for a road. See *Hesperia*, XXVI, 1955, p. 124.

[3] *Hesperia*, VIII, 1939, pp. 424 ff.; *A.J.A.*, LII, 1948, pp. 111 ff.; *Antiquity*, XXX, 1956, pp. 9–18.

Athens, Gla, *et al.*, were built, or rebuilt, about the same time to meet a common danger. The construction of these fortresses and of the trans-Isthmian wall coincides in time with the beginning of hostile incursions from the north which culminated in the Dorian Invasion. Herodotus tells the story of the two heroes, Hyllos, the son of Herakles, and Echemos, the Peloponnesian leader, who fought a duel to determine whether the invaders should gain immediate access to the Peloponnesus. Hyllos was slain, and the Heraklidae, in accordance with their previous agreement, postponed the invasion for three generations. The duel was fought at the Isthmus. In view of our discovery of a Cyclopean wall at that place, which seems to date from the end of the 13th century B.C., we may conclude that the invaders in their first attempt to conquer the peninsula were halted by the line of defence across the Isthmus. The story of the duel is poetical elaboration.

We hear next of an Isthmian fortification at the time of the invasion of Greece by Xerxes. The Greek leaders assembled at the Isthmus and decided to run a wall from the Saronic Gulf to the Corinthian. There was great urgency to finish the work before the expected arrival of the Persians. Herodotus relates how large numbers of men worked on the wall day and night, using any material that they could find: stones, wood, bricks, crates of sand. We do not know for sure what line this wall followed. Some two miles west of the Isthmian Sanctuary there is a stretch of wall about a mile long, with large towers typical of classical fortification walls. This we may tentatively identify with the wall which Herodotus describes, but repeated attempts to trace its course across the Isthmus have failed. The defences hastily constructed to ward off the Persians may have consisted chiefly of repairs to the earlier fortification.

There are no other records of an Isthmian wall from the 5th century B.C. When the Thebans under Epaminondas invaded the Peloponnesus in 369 B.C. an attempt was made to halt their passage by means of palisades and deep trenches extending from Kenkreai to Lechaion. This would indicate that whatever wall had existed earlier was not in a defensible condition at that time. Probably the Greeks contemplated a rebuilding of the fortification at the expected invasion of the Gauls in 279 B.C., but there is nothing to show that they carried out this plan. Centuries later, during the reign of Valerian, the Isthmus was again fortified at the threat of a new invasion. At that time the line of wall may have been moved further north to take better advantage of the terrain.

The wall that can still be traced across almost the entire Isthmus (FIG. 1) has been dated at various times between the 5th century B.C. and the 6th century of our era. It has now been established beyond reasonable doubt that, with the exception of some repairs of later date, the wall dates from the reign of Justinian. It is on the Peloponnesian side of the Corinth Canal, and it follows the line of the Mycenaean wall for a distance of about 300 m. At its east end it is *ca.* 500 m. distant from the Canal; midway between the two gulfs the distance is more than twice as great. The best preserved sections of the wall, near the Sanctuary of Poseidon on the east side of the Isthmus, are faced with large squared blocks mostly re-used material from ancient buildings (PLATE IX (*d*)); the intervening core consists of rubble masonry in typical Byzantine style. The construction is uniform and of excellent quality. Since the wall was constructed to withstand attacks from the north, there are towers on the north side at irregular intervals of *ca.* 40 m. Zosimus states that the whole wall had 153 towers and somewhat less than half of these can still be seen. At the east end, for a distance of some 300 m., the wall has completely disappeared, being buried beneath a layer of earth from the Corinth Canal. Where the ground was level or nearly so the wall was made more impregnable by a double line of outer defences. They consist of a low wall, only 0·70 m. thick, 6 m. from the north face of the principal wall, and of a trench, *ca.* 2 m. deep, at a distance of 3 m. from this subsidiary wall. These have been exposed in a test trench near the Isthmian Sanctuary, and where the modern road from Athens to Corinth

cuts through the wall the broad trench is still clearly visible on the north side. Such outworks, according to Procopius, constituted a regular feature of Justinian fortifications.

Near its two ends the wall was further strengthened by fortresses on the south side. Of the western fortress little is now visible, but descriptions by early observers indicate that the wall terminated in a bastion. The fortress near the east end is comparatively well preserved (FIG. 2). It consists of an enclosure of irregular shape, some 225 m. long and nearly 200 m. broad at its widest point. The Isthmian wall forms the north end of the fortress. The construction is very similar to that of the Isthmian wall, but the masonry is even more imposing and the material used is almost entirely taken from ancient buildings. The wall is preserved in places to a height of nearly 4 m. It is strengthened by nineteen towers, seven of which are on the north side and consequently are part of the Isthmian wall.

There are two main gates, one at the north-east corner, the other at the south end. The north-east gate incorporates an earlier structure, which the excavators identified as a triumphal arch of the first century after Christ. There can be little doubt that the four piers upon which the three arches rested are earlier than the rest of the wall, but a triumphal arch at the edge of the gulley so far from the Sanctuary of Poseidon seems out of place. This is one of many points still to be tested by excavation. The south gate is now blocked to a height of 2 m. A circular medallion with a large cross in relief is cut on blocks built into the wall on either side of the gate. Since the medallions are in relief the crosses cannot have been carved after the stones had been built into the wall; consequently they date the gate and the fortress in the Christian era. The Justinian date of the construction rests on literary, epigraphical, and archaeological evidence of unquestionable validity. The gate is flanked by towers, both octagonal on the outside and resting on massive rectangular podia. The interior of the east tower is circular; that of the west tower is octagonal. Both appear to have been roofed with corbelled domes. In addition to the two major gates, which probably mark the line of the principal north-south road through the Isthmus, the original wall of the fortress had sally-ports close to the towers, but these appear to have been blocked up at a later date. Until recently this fortress was commonly identified as the precinct wall enclosing the temples of Poseidon and Palaimon.

The Isthmus of Corinth, washed by the waves of two seas and frequently shaken by earthquakes, was a natural site for an early cult of Poseidon. In mythology Poseidon received the area in exchange for Acrocorinth, which became the domain of Helios and was later ceded by him to Aphrodite. Other Olympians—Demeter, Dionysos, Artemis—and several lesser divinities—Amphitrite, Galene (Calm), Thalassa (the Sea), Eueteria (Goddess of Good Seasons), Persephone, Pluto, and perhaps Herakles—shared the cult place with the God of the Sea. There was an altar within the precinct of Poseidon on which sacrifices were offered to the Cyclops. Most important after the cult of Poseidon was that of Melikertes-Palaimon, the boy-god, who was drowned when his mother, Ino-Leukothea, daughter of Cadmus, leaped into the sea in order to escape from her mad husband, king Athamas. His body was carried on the back of a dolphin and deposited at the Isthmus beneath a pine tree. Sisyphus, then king of Corinth, found the boy's body on the shore and in celebration of the funeral he instituted the Isthmian Games. This was the Corinthian version. The Athenians ascribed the founding of the games to Theseus, who was said to have celebrated the festival in honour of his victory over Sinis, the Pine-Bender. The Athenians enjoyed special privileges at the Isthmian festival and frequented the games in great numbers.

The Isthmian Games, which were reorganized as a Pan-Hellenic festival in 582 B.C., were held biennially in the spring, during the second and fourth year of each Olympiad. They comprised the usual athletic and equestrian events, as well as competitions in music, poetry, and painting. Possibly they included a regatta, for there was a legend that the first

boat-race was won with the Argo. After this victory Jason is said to have dedicated the legendary ship in the Temple of Poseidon.

Because of its central location and accessibility the Isthmian Sanctuary became a meeting place of the Greeks when the need arose to take common action. Doubtless the commercial character of Corinth and its reputation as a centre of entertainment and luxurious living had something to do with the choice of site. The Hellenic Congress of 480 B.C. has already been mentioned. After the battle of Chaironeia, when Philip of Macedon set out to create a united Greek nation, he met the representatives of the several states at the Isthmus, and two years later his son Alexander called the Greeks together there to proclaim him their leader against Persia. Corinth was to be the capital of his One World (Oikoumene). The high hopes engendered by this action died with Alexander. Even after the Greek states had lost their independence they met on occasion at the Isthmian Sanctuary to hear proclamations from their conquerors. It was here that Quinctius Flamininus proclaimed the freedom of the Greeks after his victory over Philip V at Kynokephalai in 196 B.C. Corinth was then the most important member of the Achaean League, whose interests ran counter to the imperial policies of Rome. When the Roman general L. Mummius sacked the city in 146 B.C., part of the Corinthian territory, including the Isthmus, became public domain, and control of the Isthmian Games passed into the hands of the Sikyonians. There is no literary evidence that the Isthmian Sanctuary became devastated, but the excavation within the temenos shows the effects of neglect antedating the earliest Roman reconstruction. On one later occasion the Greeks, assembled to celebrate the Isthmian Games, were offered illusory freedom. In the year A.D. 67 Nero appeared in the Isthmian Stadium and made his famous declaration, a copy of which has been preserved in an inscription from Boeotia:

> 'Men of Hellas, I am here to grant you an unexpected boon—although my generosity is such as to leave no room for surprises—a gift of such magnitude that you were unable even to ask for it: All you Greeks who inhabit the Achaea and *what has been up to now*[4] the Peloponnesus, accept (from me) freedom and exemption from taxes. This benefit you did not all possess even in the years of your greatest good fortune . . .'

and more in the same vein.

The site where the games were held has long been known, and two of the public buildings, the Stadium and the Theatre, have always been visible above ground. But before 1952 little was known of the topography, and even the location of the principal sanctuary was unknown. Most archaeologists, following Monceaux, who did some excavation there in 1883, erroneously identified the precincts of Poseidon and Palaimon with the fortress which has since been shown to be from the time of Justinian. During three campaigns of excavation by a University of Chicago expedition (FIG. 3) the Temple of Poseidon and the surrounding precinct with its two altars have been excavated, the Temple of Palaimon has been discovered, a second stadium of early date has been partly cleared, the Mycenaean wall across the Isthmus has been found, and several more distant areas have been explored.

The earliest Temple of Poseidon seems to have been built before the middle of the 7th century B.C. We have discovered large quantities of roof tiles of a very archaic type and numerous blocks from the lower parts of the walls with characteristic grooves on the under side for letting the blocks into place with ropes. These and some rather indistinct traces of the foundation indicate that the temple was of considerable size. It was burned to the ground about the time of the Persian wars, and the debris of the building and of the

[4] This expression seems to indicate that Nero contemplated a change in the name of the peninsula from Peloponnesus to Neronnonesus; see *I.G.*, VII, No. 2713, p. 478.

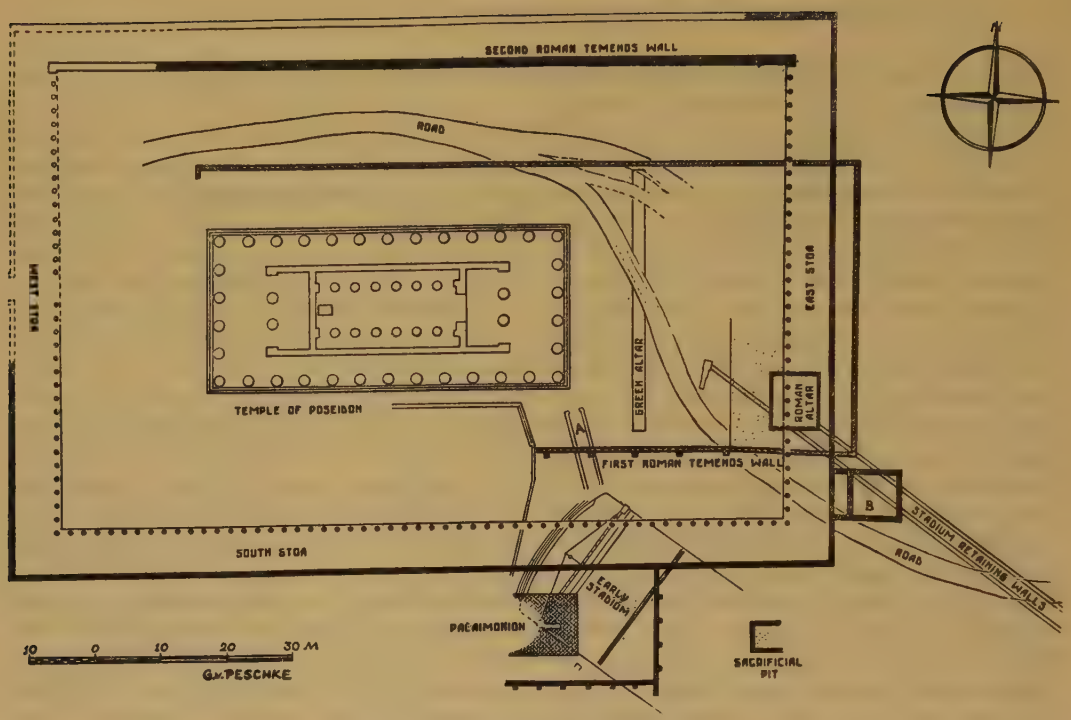


FIG. 3. Isthmian Sanctuary.
A. Stadium Entrance. *B. East Propylon.*

dedications within the temple at the time of the fire was thrown into a gulley on the north side of the precinct. A decade or two after the fire a new temple was constructed on the site of the old and this, though many times damaged and repaired, probably remained standing till the time of Justinian. It was a large Doric building, *ca.* 56 × 25 m. on the foundation, and it had six columns on the ends and thirteen on the flanks. The material was *poros*, but the roof was of marble. Pieces of marble sculpture have been found that may be from pedimental groups; they show excellent quality, but they are too fragmentary to reveal the subject matter. The 5th century temple was severely damaged by fire in 394 B.C. and had to be extensively repaired. Even the walls and the columns were largely replaced, and a new marble roof was made. The interior arrangement seems to have remained unaltered. The temple underwent several subsequent repairs, but the later history of the building cannot be established from the meagre remains. Although smaller than the contemporary Temple of Zeus at Olympia, it is one of the large temples of Greece, and Pausanias's remark that it was not very large is an unexplained puzzle.

The early altar of Poseidon was of heroic proportions. Its foundation, a little to the east of the temple, measures 40 m. in length and 1·88 m. in width (FIG. 3). The altar itself was entirely demolished before Roman times, perhaps at the time of destruction of Corinth by Mummius. In the first century after Christ the precinct was enclosed with a wall and a new altar was built which is not on the axis of the temple. In the time of the Antonines a high-priest of Poseidon, Publius Licinius Priscus Iuventianus, undertook an extensive building programme at his own expense. His donations to the Isthmian Sanctuary are recorded on two stones, one now in Verona, the other in Corinth. It is a long list, including many new constructions, both sacred and secular, and repairs to several buildings damaged by earthquakes and old age. The excavations have revealed a rebuilding and reorganization of the Sanctuary on a vast scale, dating from the period indicated by the inscriptions.

The temenos was enlarged to a total length of 125 m. and a width of 83 m., and stoas were planned to face the temple on all four sides. They seem to have been finished on the two ends and on the south side. On the north flank, where the stoa would have extended over the deep gulley that had served as temple dump, the project was apparently abandoned while the foundation was being laid. A heavy precinct wall was built later, resting partly on the foundation of the projected North stoa. The East stoa extended over the second altar of Poseidon, which would then have been demolished. No successor to this altar has been found. In the south-east corner of the precinct a propylon was built, and this seems to have been the principal entrance, through which Pausanias approached the temple. There are traces of a second gateway at the west end of the temenos.

The cult of Palaimon was second in importance only to that of Poseidon. His temple, according to Pausanias, stood within the precinct to the left of the entrance. It had a crypt in which oaths were administered that were so binding that a perjurer could in no way escape punishment. Digging in this area we found a foundation of Roman construction which can without much doubt be identified as the Palaimonion. Only the east end has been excavated. Here is preserved the descent to a passage under the floor level, which answers Pausanias's description of a crypt. Numerous lamps were found in the area in front of the temple foundation, and some of them are of a type hitherto unknown in Greece. They had doubtless been used as cult vessels in the nightly celebrations which formed part of the mystery cult of Palaimon. There must have been an earlier temple which still remains to be found.

The most surprising discovery in this area is a stadium with an intricate device for starting runners on their course (PLATE IX (*a*)). It has a shallow pit from which grooves, bridged at both ends by bronze staples, fan out towards the gates. The latter were so constructed that the starter, standing in the pit, could open or close the sixteen gates by means of cords running through the grooves (PLATE IX (*b*)). Since they were manipulated separately, the gates could be opened one at a time or all at once, as each type of race demanded. They were probably intended primarily for use in preliminary heats, by which the final contenders were selected. The stadium comes so close to the façade of the Temple of Poseidon as to leave little room for the ceremonies in front of the temple. This may have been the reason that it was abandoned and the late Palaimonion built over its east end. The new stadium was presumably then constructed in a natural hollow, some 300 m. farther south-east (FIG. 2). It is this second stadium to which Saint Paul refers in I Cor. 9. 24; and it was here Nero read his proclamation of freedom for the Greeks.

The Theatre, some 50 m. to the north-east of the Sanctuary of Poseidon, is so poorly preserved that it hardly seems worth the expense to excavate it. A trench dug there in 1954 revealed foundations of the *skene* and what appears to be the sill of a *proskenion*, but the auditorium seems to be completely stripped of its stone seats. There were many other buildings in the area surrounding the Sanctuary, and foundations of some of these are visible above ground. North of the Temple, close to the wall of Justinian at the edge of a deep ravine, are the ruins of a large Roman bath. In a trial trench dug within the foundation two rooms with hypocaust were partly cleared. There was a settlement of considerable extent on the ridge south of the Temple of Poseidon. The excavations here have exposed many house foundations, cisterns and wine presses. From coins and pottery we can date the settlement to the 4th and 3rd centuries B.C. The Sacred Glen (*Hiera Nape*), which seems to have been an important adjunct to the Isthmian Sanctuary, contained temples of Demeter and Kore, of Dionysos and of Artemis. It lay west of the Sanctuary, on the site of the present village of Kyras Vrysi. In the fields among the houses the owners have recently found a *krater* of relief ware and a statue base, both with dedicatory inscriptions to Demeter.

The Isthmian Sanctuary is the last of the great Panhellenic centres to yield its secrets to the excavator's spade. It is more thoroughly devastated than Olympia, Delphi, or even Nemea. The temples and all the buildings erected to accommodate the visitors to the Isthmian Games offered ready-made material out of which Justinian's engineers constructed the fortification across the Isthmus. Column drums from the Temple of Poseidon were rolled downhill and sliced into sections; wall blocks and other architectural members were merely trimmed to fit the space they would occupy in the wall. Lime mortar was conveniently provided by marble statuary and inscribed stelai, which were broken into fragments and thrown into the kiln. The pagan gods were dead, their houses of worship would provide protection for the believers in the new God. The builder of the fortress voiced this prayer at the end of his work and recorded it on a marble plaque:

> ' Light of Light, True God of True God,
> protect the Emperor Justinian
> and his faithful servant Victorinus,
> together with those who dwell in Greece, living in God.'

A sign of the cross concludes the prayer.

The wall built by Victorinus is the finest example of military architecture in Greece from the era of Justinian. But it did not for long protect the inhabitants of the Peloponnesus against attacks from the barbarians. Shortly after 580 the Avars overran much of the territory south of the Isthmus and spread destruction where they went. A hoard of bronze coins from our excavation testifies to the uncertainties that beset the inhabitants of those times. The owner had buried the little treasure in the ruins of the demolished Temple of Poseidon; he never returned to reclaim it. Most of the 270 pieces are coins of Justinian I (A.D. 527–65) and Justin II (A.D. 565–78), a few were minted during the short reign of Tiberius II (A.D. 578–82). These would all have been in circulation at the time of the Avar invasion.

There were other invasions into the Peloponnesus during the subsequent centuries. As late as A.D. 1415 the Emperor Manuel II found it desirable to repair the Isthmian wall as protection to his son Theodore, then appointed despot of Mistra. Until the fall of Constantinople in 1453 it remained an essential line of defence. Many armies, of Slavs, Franks, Venetians, Byzantines, Turks, Greeks, have marched over the Isthmus as conquerors or defenders. Possession of this neck of land usually—not always—meant control of the peninsula to the south of it. The heights on both sides of the Canal bear the scars of the last invasion, during World War II. Then, as many times before, the communications were severed; they were restored some years after the invaders had departed. Ships going through the Canal now keep traffic moving between the Aegean and the Adriatic; trains and vehicles speed along the lines of the ancient routes, carrying passengers and produce to and from the Peloponnesus. The Corinthian Isthmus is still a vital knot in the traffic of Greece and her neighbours.

NOTE: The earlier literature on the Isthmia will be found in Pauly-Wissowa, *Real-Encyclopädie*, IX, 2, Isthmia (K. Schneider), and Isthmos (Fimmen); and in Harold N. Fowler, Corinth I, *Introduction*, pp. 48–71. To these references should be added Jenkins and Megaw, *Annual of the British School at Athens*, XXXII, 1931–32, pp. 68–89; Broneer, *Hesperia* VIII, 1939, pp. 181–190; XXII, 1953, pp. 182–195; XXIV, 1955, pp. 110–141; and *Archaeology* VIII, 1955, pp. 56–62; IX, 1956, pp. 134–137, 268–272.

The Nature of Wansdyke

by ANTHONY CLARK

Mr Clark is a young scientist who has always had an interest in archaeology. He says that he was lucky to be encouraged and set upon the right path by the late Dr Crawford, as well as by Sheppard Frere and R. G. Goodchild. At 17 he was excavating Romano-British pottery kilns at Tilford in Surrey. Since then he has done much field-work and excavation in Surrey and Wiltshire. As his article shows, his investigation of Wansdyke began with a summer walk across England; there followed excavation and field-work for four years, with the results summarized below.

'THE middest of this shire [Wiltshire] which for the most part lieth plain and even, is divided athwart from East to West, with a dyke of wonderfull work, cast up for many miles together in length: the people dwelling thereabout call it Wansdyke, which upon an error generally received, they talk and tell to have been made by the Divell upon a Wednesday.' So wrote John Aubrey in 1663 in his *Monumenta Britannica* and, since his day, Wansdyke, one of the two mightiest linear earthworks of ancient Britain—the other is Offa's Dyke—has rightly attracted the admiration and speculation of innumerable antiquaries (PLATE X (*a*)).

Since the earliest attempts to explain it, all sane writers on Wansdyke have agreed that it is a boundary built along their northern frontier by a people occupying the once coveted hills of Wessex, for the ditch is always on the north side of the bank and, when following hills, the Dyke hugs with great tactical care the north side of the ridge in order to command the ground in that direction. The terminal points, 46½ miles apart as the crow flies and almost due east and west of each other, are the Iron Age hill fort of Maes Knoll, crowning the east end of Dundry Hill, from the foot of which the southern suburbs of Bristol now extend; and on the east, a field in the parish of Inkpen, Berkshire, under the tree-hung brow of the North Downs ridge where it has just disengaged itself from the Wessex chalk massif to begin its long march to the Dover Strait. Between these points the Dyke has been most prominently observed in Somerset running east from Maes Knoll, parallel to and south of the Avon to the head of the Horsecombe Brook, two miles south of Bath; and in Wiltshire between Morgan's Hill and Savernake Forest, along the bare ridge of the Marlborough Downs that defines, with breathtaking nobility, the northern boundary of the beautiful Vale of Pewsey. At the western end of this stretch, the Dyke runs into the Roman road from London to Bath, whose alignment it clearly assumes, and it has been generally accepted that Dyke and road are coincident onward for seventeen miles to the neighbourhood of Bath, where some sort of link with the Somerset section has been presumed. Wansdyke is noted for the inclusion of earlier hilltop earthworks in its structure. In Somerset there is Maes Knoll, and Stantonbury further east; and in Wiltshire the Dyke, after being interrupted by Savernake Forest, issues forth afresh from the hill fort of Chisbury on the east side of the Forest. This ' stupendous boundary called Wansdyke ' attains a total width of about 80 ft. and a rise of 25 ft. from the bottom of the

ditch to the rampart crest on the Marlborough Downs, and the dimensions are similarly impressive in Somerset.

In theorizing about the date and origin of the Dyke, authorities of the past have been reasonable but divided. Aubrey, on the basis of the (correct) derivation of the name from that of the Saxon god Woden, suggested that the Dyke was the boundary between the kingdoms of Wessex and Mercia, but Stukeley, writing in the early 18th century, regarded it as the northernmost of four boundaries of the expanding Belgae, and he gave a Celtic derivation for the name from the Welsh *gwahan*, to separate. The great Wiltshire historian, Sir Richard Colt Hoare, whose survey of the Wansdyke [1] published in 1821 has remained one of the most intelligent up to the present day, with his antiquarian collaborators Leman and Skinner, combined these two theories and imagined Wansdyke to have been a Belgic boundary subsequently enlarged by the Saxons. Colt Hoare was therefore very triumphant when a section through the bank of the Dyke, revealed by a cutting for a new sheep-drove east of Morgan's Hill, seemed to show that there were indeed two superimposed banks of different ages. It fell to the lot of General Pitt Rivers, the inventor of modern excavation, to discredit the apparent evidence of this section and to obtain the first and so far only direct evidence for the date of the Dyke.

Pitt Rivers cut three sections through the Dyke in 1889 and 1890.[2] His Section I, on Morgan's Hill, was a tremendous trench 30 ft. wide, taken down to the chalk bedrock across the whole width of the Dyke, which is here at its greatest magnitude. It revealed the dark band that Colt Hoare imagined to be the turf-line of the earlier bank but the General considered that its thickness did not vary as that of the turf-line of a bank normally does, and a 30 ft. transverse section across his trench showed that the dark band was irregular and discontinuous. He therefore concluded that it was an accidental effect in the casting up of the spoil. Romano-British pottery was found in and under the bank, showing that Wansdyke was late Roman or post-Roman, and this was confirmed by his Section III, at a place he called Brown's Barn, 1·7 miles east along the Dyke from the Devizes to Marlborough road, which revealed the counterscarp of the Dyke overlying the bank and ditch of a small Romano-British enclosure. Section II, across the bank of Wansdyke only, was also cut here. It again showed Colt Hoare's dark band, this time so full of sherds of Romano-British pottery that the conclusion that it had been scraped up from the surface subsequently to Roman-period occupation was inescapable.

Twentieth-century writers have thus been left with three possibilities to play with: late Roman, sub-Roman and Saxon; but since Leeds and others demonstrated that the first Saxon settlements were probably in the upper Thames valley, the Dyke, or at least its eastern part, has generally come to be regarded as most probably a sub-Roman barrier against their southward expansion over the chalk hills into the heart of Wessex.[3] The arguments for this have been most succinctly summarized by Dr Gordon Copley in his book *The Conquest of Wessex in the Sixth Century* (1954). But there has been one considerable obstacle to the general acceptance of Wansdyke as a purely defensive work: the 17-mile central section along the Roman road from Morgan's Hill to Bath. This not only makes the Dyke so long that one seriously doubts whether it could have been safeguarded effectively by a small population, but is so placed as to force some to the conclusion that the Dyke was but a political boundary, in spite of its character on the Marlborough

[1] *Ancient History of Wiltshire*, II, pp. 16–33, where he quotes the views of Aubrey and Stukeley (from *Itinerarium Curiosum*) at some length.

[2] *Excavations in Bokerly Dyke and Wansdyke*. See also Crawford, ANTIQUITY, VI, 1932, pp. 349–50.

[3] Leeds, *Ant. Journ.*, XIII, 1933, pp. 249–51.

Downs.[4] This line no longer follows hill ridges, and it connects awkwardly with the Somerset section; worst of all, it cuts across a great loop of the Avon where the river is well developed, and its defenders would have been isolated from the main part of their territory with all lines of retreat cut by the river. But Wansdyke here seems well authenticated. Ancient charters and local usage have preserved the name in several places, and Leman and more recent investigators claim to have definitely observed it in Neston and Spye Parks, so it is small wonder that the Ordnance Survey have always marked the line as ' Roman road and Wansdyke '. A book published in 1926, the charmingly illustrated *Mystery of Wansdyke*, by Major and Burroughs, also takes the Dyke along this line and, with limitless misguided detail, traces extensions of the two ends to the mouth of the Avon and to Ludgershall in Wiltshire, extensions which do not stand up to archaeological scrutiny. The best modern survey is by Crawford in his book *Archaeology in the Field* (1953), pp. 252–7, which should be read with his article on the Dyke's eastern end in *Wilts Arch. Mag.* No. 199, pp. 119–25.

* * *

In August, 1953, I set out on a holiday walk from Streatley on the Thames, following the ancient ridgeway ultimately coming from East Anglia, the lofty green track along the north rim of the Berkshire Downs above the Vale of the White Horse, curving ever more southward until it passes Avebury and crosses the line of Wansdyke on the Marlborough Downs. Here I left the ridgeway to follow the Dyke westward along these downs where, as Crawford has said, ' it runs in almost undiminished magnificence '. I had decided to follow the Dyke all the way to Bath and, when it joined the Roman road, I pursued the course of the road. Now I was in country where cultivation had, over the centuries, removed much of the evidence of antiquity; but the arrow-straight Roman line is almost continuously marked by hedgerows beside which, here and there, I thought I could see traces of Wansdyke. Near the Avon, the line enters the last piece of untouched country before Bath, the fine primeval stretch of sandy heath which is Spye Park. Here the Roman road encountered a steep little stream valley running from north to south, and zig-zagged northward into it to avoid excessive gradients. Now, if Wansdyke is a military work, as my view of it over the Marlborough Downs had convinced me it was, then here was an impossible situation for it—if it indeed stood upon the road, as my Ordnance map bade me believe, then it would be here utterly out of character, facing indefensibly up the precipitous sides of this valley. I searched for the Dyke on more likely ground, but there was no trace, and I had to push on.

Beyond Spye, the Roman road descends to cross the Avon. In the descent, a deep ditch, marked as Wansdyke on the Ordnance six-inch map, is on the line, but here we have passed the spring-line and are on clay, and this hollow is no more than a watercourse worn by a healthy stream embanked by the Roman road. Beyond the river and just after the railway, the Ordnance surveyors had chosen a mere lynchet to represent Wansdyke, and a mile farther on the Roman road hangs so precariously on a southward slope that northward defence would again have been impossible. Now, for 6 miles to Bathford, the road ran and I clambered over a subsoil of oolitic limestone, and any ditch cut for Wansdyke should have silted but little, and would certainly have been preserved here and there; but no ditch was ever to be seen although the Roman road, for instance at Norbin Farm in Box parish, was sometimes well defined. As I tramped wearily into Bath in the evening of the fifth day of my walk, I knew that I had stumbled upon an exciting archaeological

[4] *E.g.* Sir Charles Oman's essay on Wansdyke in *Arch. Journ.*, LXXXVII, pp. 60–70.

problem, crucial to the whole character of Wansdyke: what had become of it across the loop of the Avon? The investigation was to absorb my spare time for four years.[5]

What *had* become of the Dyke? My first clue came from Crawford's survey, in which he had decided that the evidence for the generally accepted continuation of the east end of the Somerset portion northward past Bath towards the Roman road at Bathford was too slender to be accepted, and he preferred to leave the eastern end resting upon the head of the steep valley of the Horsecombe Brook which, *via* Midford Brook, flows into the Avon just below Bradford and 3 miles south of Bathford. Having thus apparently created the prece-

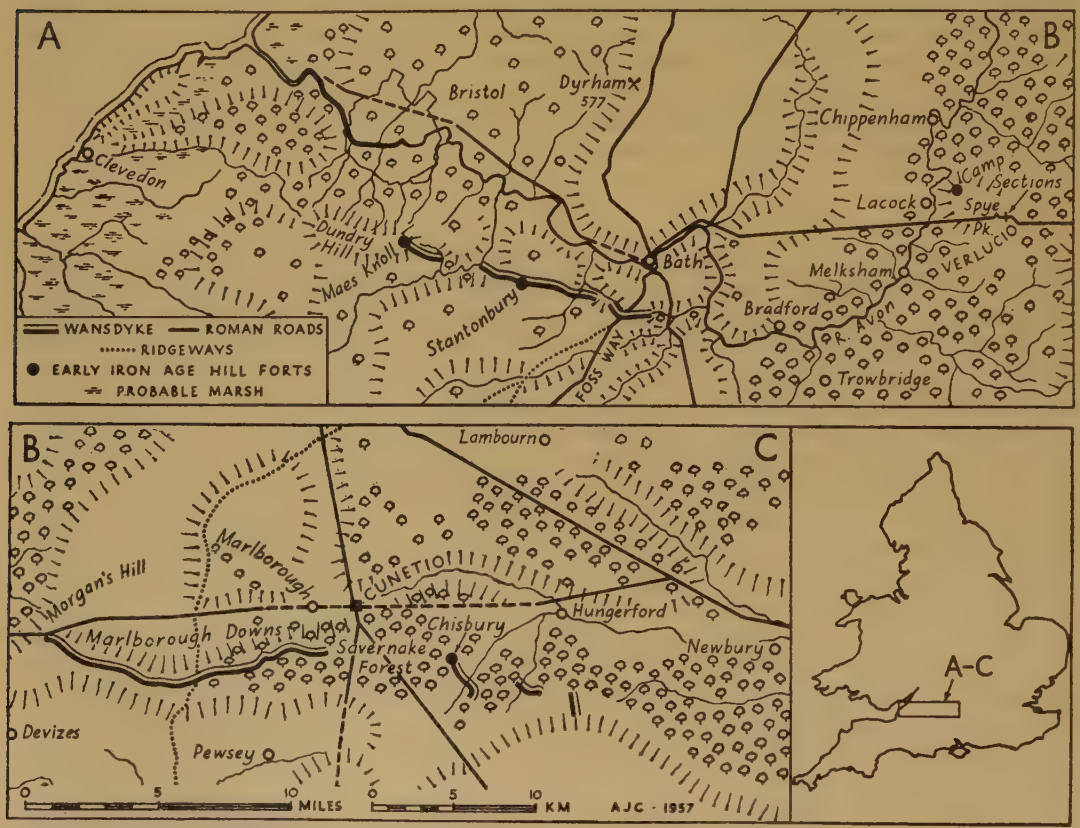


FIG. 1. Wansdyke in Somerset and in Wiltshire.

dent of following stream valleys, the Dyke, it seemed to me, could far more logically be expected to continue approximately the line thus established by following the Avon upstream, than by indulging in a huge 3-mile jump and then pursuing a tactically bad course isolated from its main territory by the very loop of the Avon it seemed to run into so naturally. If, then, the Avon was the course, it was my job to link the Wiltshire part of the Dyke with the river in the neighbourhood of Lacock.

[5] I later found that Colt Hoare (*ibid.*, p. 28) had been similarly dubious about this part of Wansdyke. Regarding the Morgan's Hill junction he wrote: 'This junction of two works so dissimilar in their construction and course, has always appeared to me so very unnatural, that I have used my utmost endeavours, both by repeated enquiry, and personal investigation, to rectify this apparent contradiction, but I have never been successful in procuring any tidings or even hint, of a continuation of Wansdyke after its well known junction on Calston [Morgan's] Hill.'

The reader will remember that I had followed the Roman road into Spye Park, within which the course seemed to become defensibly untenable. Spye Park stands on the only greensand outcrop in Wiltshire, the imposing bluff of Bowden Hill, 589 ft. high, abutting directly upon the Avon, across which lies the village of Lacock. If, as I imagined, Wansdyke had left the Roman road in or just before Spye Park, then it could have commanded the northerly slope towards Chippenham just as effectively as on the Marlborough Downs by following a course on the north side of this ridge, connecting with the river in a short half-mile plunge at the end. Consultation of the maps showed a belt of trees called Beech Walk, accompanied by a narrow modern road, which exactly filled this condition and which incidentally marks the route of the old Bath Road. Field-work showed that Beech Walk

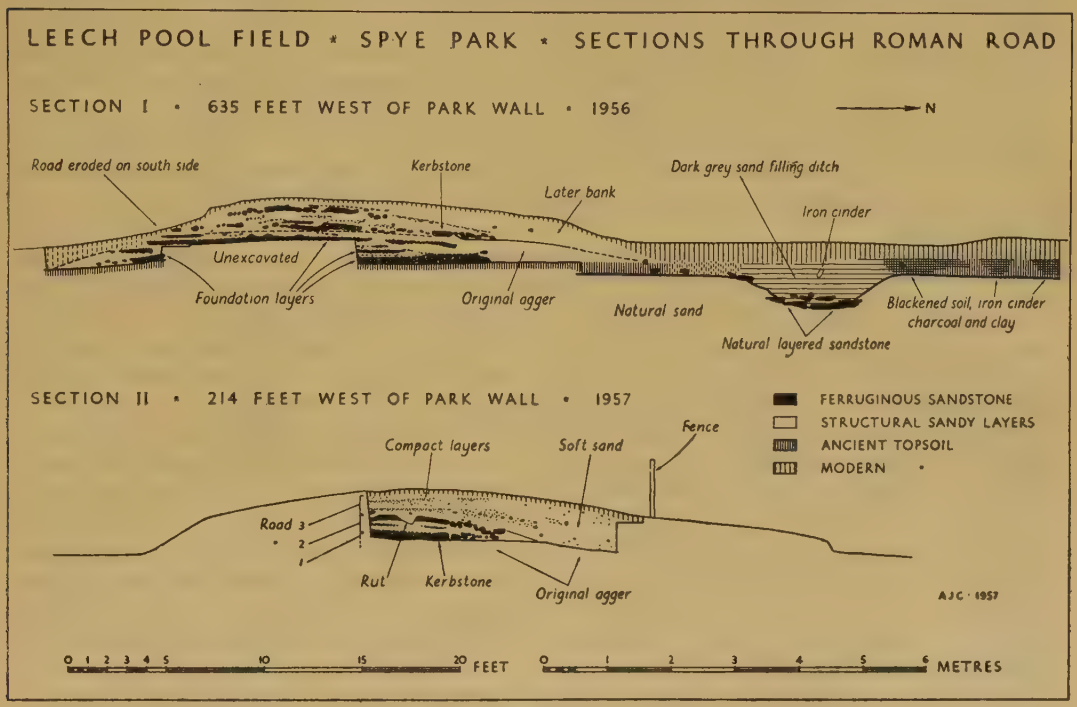


FIG. 2.

was quite heavily banked on both sides, and I was prepared to concede coincidence between the Dyke and the old road, for it behaves in this manner elsewhere. Most remarkably, however, I found that at the north-west end of the ridge, above the river, this line runs into a fine promontory camp, the wide rampart and ditch of which I discovered isolating the tip of the ridge, where it is called Nash Hill, and in the camp I found Roman pottery. Surely, I thought, this camp, like Maes Knoll and Chisbury, must represent the terminus of Wansdyke hereabouts, and I confidently set about excavation in Beech Walk in the hope of picking up the filled-in trench of Wansdyke. For four long weeks in two hot summers my stout friends and I laboured, but in the end we had to admit defeat. In 1954 we sectioned the embankment on the north side of Beech Walk, but this was not substantial enough; in 1955 we cut across the old Bath Road itself, and to our temporary joy discovered a great V-sectioned dark pocket in the sand—but this proved to be a geological feature!

The Beech Walk line was disproved, the Nash Hill camp a red herring. In 1956 we were forced to retreat intellectually and geographically—to resort to an attempt to disprove the coincidence of Dyke and Roman road in Spye Park (FIG. 2). Two sections 4 ft. wide were ultimately cut, in 1956 and 1957, both in the Leech Pool field which is just inside the east wall of the Park, by Sandy Lane village. The road *agger* is here particularly high and impressive. Section I showed the road to have an interesting structural sequence. Initially it had consisted of a modest *agger* of rather clayey sand, reinforced with layers of ironstone rubble. Placed on the north side of the *agger* (and presumably also on the south side which had, however, been eroded) was a kerb of contiguous ironstone slabs about a foot square, defining the edge of the road. Later more rubble and sand were deposited on this road, by then much mutilated, and a new surface was laid, spreading over the old kerb. The section was extended north of the road in case the ditch of Wansdyke lay there, and a ditch was indeed encountered, though far too trivial to be that of the Dyke. Throughout its filling were fragments of charcoal and iron-bloomery cinder that had spilled in from the north since soon after the ditch was dug. The ditch and the iron-working were thus clearly contemporary, although it is uncertain whether they are Roman or medieval in date; but whatever the date, they tell together a story of peaceful activity alongside the road, not of a frontier line. Trial trenching on the south side of the road did not reveal a second ditch.

One disquieting feature of this section was the appreciable bank of sand overlying the road on its north side. It was not impossible to interpret this as the last relic of the bank of Wansdyke, but nearer to the Park wall the Roman road is preserved to a considerably greater height and is hard just below the turf when probed. This suggested to me that the road, evidently from Section I a narrow and steep-sided affair in its first reconstruction, had ultimately been made safer and more appropriate to its role as the main road from London to *Aquae Sulis* by a substantial widening of the *agger* and resurfacing, and that in Section I the superimposed bank represented a last remnant of this. The partial Section II was cut in 1957 in the higher part of the road 417 ft. east of Section I to check this theory, and did in fact confirm it. The section was taken down only to the top of the first *agger*: the kerb was again found, again overlain by the later surface, this time well preserved and with a heavy rut. Above this we found what we were looking for: about a foot of compactly layered small ironstone gravel and sand, impenetrable to a probe and requiring a pick to break it up, supported by and to some extent overlying the soft sand bank that we had encountered in Section I.[6]

By the time the cutting of these trenches had been started, Sir Cyril and Lady Fox, who in their own general survey of Wansdyke had confirmed my negative discoveries, rounded off all this laborious research with one brilliant piece of field-work: the discovery of what must be the tail-end of the Dyke 4 miles away on Morgan's Hill itself, only 300 yards from its junction with the Roman road! They observed that, after the junction, the Dyke had been constructed by digging out the Roman road terrace and casting it up on the hillside above, to the south. But just as Smallgrain Plantation is reached, the ditch swings northward and away from the Roman road for a few feet and then stops. Similarly the bank suddenly appears as an isolated mound in the hedgerow on the north side of the road. A quarter of a mile farther on, the *agger* of the road appears to be intact. As simple as that! This surprising termination is upon a dry chalk subsoil in country which is now quite open, but clay is less than a mile away and we must assume that the fringe of Selwood Forest extended as far as this when the Dyke was built. But how could reliance be placed upon mere woodland as a substitute for the mighty rampart on Morgan's Hill? Sir Cyril

[6] I hope to publish the results of my work in this area more fully in the *Wilts. Arch. Mag.*

Fox has suggested to me that the commander of an ill-disciplined group of settlers could only have controlled his horde if the whole of it was in view all the time: in a thick wood the army would have disintegrated. The great ditches of the Belgic *oppida* at Colchester, and the newly-discovered Bagendon, near Cirencester, ancestors of Wansdyke itself, clearly exploit woodland in this way; and Caesar, in his description of the second invasion of Britain in *De Bello Gallico*, gives a lively account of the use by the British cavalry of woodland cover for surprise attacks.

Such a termination upon woodland seems, furthermore, to be in keeping with the general character of Wansdyke. At its eastern end it has never been found convincingly in Savernake Forest, and the three disconnected lengths across chalk and alluvial corridors through the naturally wooded clay country beyond the forest seem never to have been linked (see FIG. 1). In only one area does the Dyke run for an appreciable distance over such ground—the deposit of clay-with-flints, which is still wooded, on the chalk west of Savernake, where the lonely seam of the Dyke through the bare trees of winter is a strangely moving sight from the air; but here we are only a mile or so from the danger of the ridgeway and an exception can be expected. Crawford has shown that in Somerset every opportunity was taken to avoid construction of the earthwork where steep and wooded stream valleys could take its place, and in resting its western flank on Dundry Hill the builders were clearly relying on the defensive value of the country between there and the Severn Estuary, where wooded hills and vales intersected by a maze of deep-cut streams make going difficult even at the present day, and closer to the coast much land was probably made impassable by a marine transgression that seems to have occurred at the end of Roman times both here and elsewhere.

The supporters of a continuous Wansdyke have tended to focus their attention on Neston Park, in the loop of the Avon, where the Roman road was of unusual height before being reduced for landscaping reasons in the early 19th century. In Devizes Museum there is a manuscript description of the Dyke by Leman, transcribed by Colt Hoare, showing a section of the road here with a great bank piled upon it; but Skinner, writing at the same time, gives a pair of more carefully drawn sections that show the earthy layer with more modest proportions (perhaps 18 in. thick), overlain by small stones and gravel, which could well represent a variant of the final version of the road as I found it in Spye Park. Skinner's description, a most useful one, is in one [7] of the ninety-eight of his manuscript volumes in the British Museum. His first section was drawn from an isolated chunk of about half the width of the road left to its full height of about 4 ft. after the landscaping. That mound still stands, the young oak that Skinner saw upon it now a mighty veteran, and his upper stone layer is still visible (PLATE X, (*b*)). Having this evidence that the Roman road was very massive in its final form, the persistence of the name Wansdyke hereabouts need not imply the existence of the later earthwork. The Roman road called Ackling Dyke in south Wiltshire and Dorset seems to be a parallel case. My hypothesis that the Avon loop formed the central part of a continuous Wansdyke must also be abandoned because of Sir Cyril Fox's discovery of the termination so far from the river on Morgan's Hill, although there seems little doubt that the river formed the eastern end of the Somerset Dyke.

Wansdyke, then, was not a vast project of fanatical and often futile earth-moving, perhaps even so with only political demarcation in mind, but was a defensive frontier planned with economy and skill to block the two open corridors into ancient Wessex from the north-east—the chalk in Wiltshire and the Jurassic belt whose ridgeway coincides approximately with the Foss Way south of Bath and north of it was Romanized past

[7] B.M. No. Add. 33, 654.

Dyrham. Thus, in spite of the 46 odd miles separating the terminal points of this defensive system, the total length of earthwork constructed seems to have been but 15·2 miles in Wiltshire and 7·5 miles in Somerset.

I say ' defensive system ': but was Wansdyke a single system ? Relieved of the necessity of regarding the Dyke as a whole, it may be a less Procrustean enterprise than in the past to try to fit its components into the obscure picture we have of events in the 5th and 6th centuries. Copley argued well that the Wiltshire Dyke could best be put in the late 5th century, but Saxon penetration to the lower Avon was much later,[8] making the Somerset Dyke difficult to explain. Myres relegated to a footnote [9] the suggestion that the latter, running as it does south of the far more natural frontier of the Avon and excluding Bath, was built after the battle of Deorham (Dyrham) in 577 in which Ceawlin and Cuthwine, according to the Anglo-Saxon Chronicle, captured Cirencester, Gloucester and Bath; but this idea now seems more strongly tenable. A dyke following an often indefensible course on the dangerous side of the Avon, along a Roman road running into Bath itself, could not possibly have been fitted into the post-Deorham picture, but Crawford's Somerset Dyke fits perfectly. Furthermore, Sir Cyril Fox, in yet another acute observation, has noted that whereas the Dyke in Wiltshire follows the ideal defensive course, in Somerset the builders, denied the river line by the occupation of Bath, had to pick a much less satisfactory course among the hills: the line was dictated by the enemy, not by the defenders.

In 552 Cynric's West Saxons took Sarum, and then, or soon after, resistance south of the Wiltshire Wansdyke must have ceased. Not long after 577, we may imagine the Britons confined west of the forest of Selwood, the entries to their country securely trenched across by the Somerset Wansdyke in the north and the other great dyke of Bokerly in the south-east. It seems that this frontier was not breached until the 7th century.

* * *

I must record my warmest gratitude to Captain Frank Spicer, D.S.O., owner of Spye Park, for permission to excavate, and for his helpfulness and enthusiasm; to my tireless diggers, David Baker, Richard Comber and John Mead; and, for hospitality, encouragement and assistance, to John Baker and Malcolm McCulloch, and to Nicholas Thomas and Kenneth Annable, past and present Curators of Devizes Museum. My debt to Sir Cyril and Lady Fox should be apparent in my text.

[8] Unless we accept Leeds's suggestion (*ibid.*, p. 233), based on some statements of Gildas, that the battle of Mons Badonis (A.D. 490–516) was at Bath, in which case the Somerset Wansdyke could well belong to this period. But many places contend etymologically for the honour of being the site of this devastating defeat of the Saxons by Arthur: perhaps as good a case as any can be made for Liddington Castle (once Badbury) on the Berkshire Ridgeway. See Brentnall, ANTIQUITY, XV, 1941, pp. 43–4.

[9] *Roman Britain and the English Settlements* (1945), p. 403.

The Linear A Question

by MAURICE POPE

When ANTIQUITY *published in September, 1957, an article by Dr Cyrus Gordon entitled ' Notes on Minoan Linear A ', the late Editor, Dr O. G. S. Crawford, hailed it as a major discovery—and so it was, if correct. In his second article* (ANTIQUITY XXXI, 1957, 237) *Dr Gordon assumed that the language of the Linear A documents is Akkadian and none other. But there are other views, and we publish here an article from the Professor of Classics at Cape Town University which he originally described as ' Linear A not deciphered '.*

DR GORDON has, in my opinion, not begun to prove his case and it is difficult to know where to begin its refutation. In at least one major respect the two articles he has written in ANTIQUITY disagree with each other. In the first article (ANTIQUITY XXXI, 124) the frequent sign L 98 was taken (on the basis of the similarity of the Linear B sign) to represent *ku* or *gu*. With the help of this value three words have been recognized as Semitic. But in the second article (ANTIQUITY XXXI, 237) the value of the sign is stated as an unknown 'X', and then, in order to make it possible to identify a word for 'cumulative total' as the Akkadian *kitmuru*, it is allotted the sound *mu*. We are not told what we must now think of two other words previously identified on the assumption that the sign stood for *ku*.[1] Nor is it made clear on what grounds one may pick and choose among the phonetic values of the Linear B signs. L 98 is certainly the same sign as the Linear B 81 *ku*; L 22 is the same sign as the Linear B 2 *ro*. Why are we justified in assuming that the one phonetic value is transferable and not the other? This point is quite fundamental in any attempt to decipher Linear A by using the Linear B phonetic values, but Gordon ignores it completely. And not only here. By identifying the supposed word for ' cumulative total ' as *kitmuru*, he commits himself to the value *ki* for the first sign L 21. As this sign hardly, if ever, occurs elsewhere he cannot check his assumption, though it compels him to alter the value of L 103 (= Linear B 67 *ki*) to *ku* (para. 6, p. 239). Allotting the value *tu* to L 39 in the same word leads to a similar difficulty, since the value *tu* is already occupied by L 6 (accepted by Gordon in the first article, para. 10). To justify so much general post the reader may assume that there must be something particularly certain about the identification of the word for ' cumulative total ' and its interpretation as the Akkadian *kitmuru*. But the word occurs only once in the whole of Linear A. And more than this. The first sign occurs practically nowhere else, and the second is extremely rare. It does not seem worth while undermining so many phonetic values for the sake of accommodating such an uncertain word.

But to revert to the equation L 98 = *mu*. This sign occurs in thirty-five Minoan words. Gordon says that he has re-examined every occurrence of the sign, and that ' none of

[1] The Minoan totalling word, which includes this sign, is now interpreted by Dr Gordon as *muru*, an Akkadian word for ' received '. This is a less likely meaning for the Minoan group than ' total ', among other reasons because it terminates lists of people as well as lists of commodities. It is also a particularly surprising transliteration for Gordon to adopt, since it deprives the Semitic hypothesis of one of its strongest arguments. The customary reading *kuro* or *kulo* could well be taken to represent the common Semitic root *kl* = ' all '.

the occurrences of L 98 run counter to the reading *mu* and one confirms it '. What can this mean ? That he has an appropriate Akkadian word to suggest for all the thirty-five words which contain the syllable ? If so, he should not withhold them from us. If not, it is hard to see how he can tell when an occurrence of the syllable is running counter to his suggested value and when it isn't. Nor is the ' one instance which confirms it ' any more encouraging. It is not even a word. It is an L 87 with a small L 98 written above it. This sort of ligature is a normal enough formation in Linear A. It sometimes happens that a certainly non-phonetic ideogram, say for CORN, is combined with a phonetic symbol written small. More rarely one finds ligatures of two phonetic syllables. In neither case can we be sure that the resulting ligature represents a word. If it did one would expect it to be spelt out normally. And yet this most uncertain instance is the only positive support Gordon can find for allotting the value *mu* to L 98. But even if we grant that the ligature L 87–L 98 forms a complete word, there is no confirmation for Gordon's transliteration of it. He says that L 87 stands for *u* (a value usually allotted to L 97, but Gordon does not mention this), and that when the sign stands alone its meaning will be ' and '. The same evidence would prove that the signs L 1, L 51, L 52, L 78, L 100 all meant ' and '. But let us grant this value too. Then the ligature L 87–L 98 will read as the word *umu*, which Gordon claims as the Akkadian for ' day ' in ' date formulae '. There is not a scrap of evidence to show that a date formula is to be expected in the two tablets where he posits it. And to support it he has to force an innocent ideographic sign which looks as if it was a schematic drawing of a loom, into representing a disyllabic or trisyllabic word for ' month ', and to pretend that what have been time and time again shown to be fractional signs are after all ordinal numerals.

So much for the dubious *kitmuru* and the disastrous consequences of reading it. But what Gordon claims as the major ' clue to the identification of the language ' within the Semitic group is the word *ka-pa* (L 29–L 2) which he interprets as the Akkadian *gabba* = ' all ' (second article, para. 1). The word is frequent as a heading. In HT 94b (cf. HT 100) it occurs before the ideogram for MAN and the numeral 62. There follows a list of other signs and numerals, which is totalled. Gordon seems to think it beyond question that ' all the men 62 ' is a probable meaning for the group. I cannot see why. It might equally mean ' For Siberia–MEN 62 ', ' To be paid–MEN 62 ', or a host of other things. But if the meaning ' all ' does not seem a necessary one when the word occurs as a heading it is almost impossible in the occurrence of the word in a list (HT 8b). This entry, which Gordon ignores, would read 'all–1/2' and it occurs in the middle of a series of words followed by numerals. This can hardly be called satisfactory sense. Incidentally *ka-pa* MAN 62 at the beginning of a list is not ' exactly equivalent ' to the Mycenaean totalling formula *to-so* MAN 17 which normally occurs at the end of lists and where the number is clearly a total of the previous entries. Nor will the reader be surprised to learn that the interpretation of *ka-pa* = ' all ' conflicts with the next identification offered (second article, para. 3). The Linear A *te* is equated with the Sumerogram TI (a questionable procedure), which is said to mean ' he received ', on the grounds that it always stands after a personal name. But in HT 6a it stands after *ka-pa da-ta-ra*, which—according to Gordon—should mean ' all the -s '. An unlikely name, even for a Minoan.

To summarize. Dr Gordon's second article has two main points. First is the identification of *kitmuru*. If this is accepted, some of the translations offered in the first article are swept away without replacement. But worse than this. For acceptance implies that the same signs in Linear A and Linear B may have quite different phonetic values. And this would invalidate the whole principle on which Gordon has been identifying words. The second major point is the recognition of the language as specifically Akkadian by interpreting

ka-pa as *gabba* = ' all '. But in HT 94 (as in HT 100, 102, 105) this is neither a necessary nor a very probable meaning for the context, in HT 6 it appears to contradict another of Gordon's arguments; and in HT 8 it is to all common sense impossible.

Finally I should like to put on record as concisely as possible a selection of the remaining serious mistakes in the two articles.

Para. 8, p. 126. The reading on HT 88 is L 52–L 93 (*a-du*), ' Bakers ' therefore have nothing to do with the tablet.

Para. 9, p. 126. The reading of HT 85 is not *a-du-si-si*. The first word is *a-du*, a frequent heading. This is followed by a double sign, which is almost certainly ideographic. It occurs again on HT 97, and in single form on HT 27 and HT 89. In any case the sign bears little resemblance to the Linear B *si*. There is no ' EQUINE determinative ' in line 5 of the reverse, or elsewhere on the tablet. There remains therefore no evidence of horsiness in the tablet, and all the suggested interpretations of other words based on this supposition fall away.

Para, 10, p. 126; para. 11, p. 127. *Ku-pa$_3$-na-tu may* mean ' harness ' and *ku-pa$_3$-uu may* be a personal name. But it seems much more likely that we have here one of the rare instances of a Minoan inflection.

Para. 12, p. 127. The occurrences of *sa-ra$_2$* in ration tablets do not suggest much affinity with an Akkadian king (*sarru*). There are numerous Semitic words which match these consonants. One could extract any of the following meanings, ' ask ', ' reject ', ' deficience ', ' riches ', ' begin ', ' grow '. This list may serve to show the impracticability of trying to decipher by vague resemblances of random words.

Para. 13, p. 127. Again Dr Gordon shows that he is not acquainted with the literature on Minoan fractions.

Para. 9, p. 239. The Knossos cup inscription (Kn 7) does not begin with the word *a-ka-nu*, but with *a-ka-nu-we-ti*. The similarity with the Semitic *aganu* ' bowl ' is interesting, especially as the word is used for a ritual basin in Exodus XXIV, 6, and the Knossos cups are likely to be of religious or magical import (see Marinatos *Minos* I, p. 40–1). But the form would be a plural, and it is not easy to see why you should write an inscription on one bowl which began ' Bowls . . .'

This ends the catalogue of sins. We now come to the virtues. The only identifications which seem at all solid—that is to say where the contextual evidence for the expected meaning of the word is strong, where the phonetic values of the Linear A signs are beyond dispute (once granting the transferability of the Linear B values), and where the Semitic identification suggested is plausible in itself—are concerned with the pot-names on HT 31. But this is just the sort of vocabulary which could easily have been borrowed. It can not therefore prove anything about the nature of the language. In any case there are only three or four such words suggested. Not even the most enthusiastic proponent could call them evidence of a decipherment.

But I would not like to conclude by leaving the impression that Dr Gordon's mistakes prove that the language is not Semitic. I can see no *a priori* objection to the presence of Semites in Crete in the M.M. III–L.M. I period who adapted the native Cretan script to their own language. And in fact there is rather more linguistic evidence for the case than Gordon has put forward. But perhaps the best reason for refusing to be persuaded by it is that Dr Gordon, who is a professional Semiticist, cannot, even with his heart in the matter, produce a credible translation of any one tablet or any one religious inscription.

The Morley St Peter Hoard

by R. RAINBIRD CLARKE and R. H. M. DOLLEY

What seems to be the largest hoard of Late Saxon coins recorded from Britain since 1872 was found recently in Norfolk. It will be published in full in due course; here the Curator of the Castle Museum, Norwich, and Mr Dolley, of the Department of Coins and Medals in the British Museum, present us with a preliminary account of this major hoard of Anglo-Saxon coins.

(*a*) The Discovery

THE significance of the find lies both in its size and in its spanning of the late 9th and early 10th centuries, a period earlier than that embraced by the very few other Late Saxon hoards from this area. Its importance is further enhanced by the fact that it is believed to have been recovered in its entirety, whereas a majority of hoards of this period are feared to have suffered some dispersal at least before they could be recorded and assessed by numismatists.

The new hoard has come to light in the grounds of Wymondham College in the parish of Morley St Peter some twelve miles south-west of Norwich (Nat. Grid. 62/076984). During January 1958 trenches were dug for new foundations, but heavy snow impeded progress, and its melting caused the side of one trench to collapse. When two bricklayers began to clean up the trench on the morning of 27 January, fragments of a broken pot and a shower of silver ' discs ' fell into the trench between them at a depth of 2 ft. 3 in. To the finders the coins looked like ' mineral-water bottle caps ', but the men noticed that some bore a portrait, and sought the help of one of the masters at the College. He at once appreciated their importance, and, suspecting that they were Saxon coins, conveyed 874 of them to the headmaster, Mr R. V. Metcalfe, who promptly telephoned news of the discovery to the Castle Museum, Norwich. The writer asked for the coins to be brought to the museum for identification, and this was arranged by the headmaster who thus ensured that the great mass of the hoard together with fragments of the container reached the Norwich Museum within three hours of being found. A rapid examination established that they were indeed Saxon silver pence, predominantly of the reigns of Alfred the Great and of Edward the Elder, and the Norfolk Constabulary was at once notified. The local police immediately visited the site and satisfied themselves that all the coins found had been reported, and thus remarkably prompt action by workmen, College staff and police has ensured that the whole hoard is now available for study and interpretation.

On the following day Norwich Museum archaeological staff visited Morley to survey and photograph the site, and further examination of the spoil from the trench resulted in the recovery of nine additional coins and of further fragments of the container. The pot, a wheel-made jar of Thetford ware, has since been reconstructed (FIG. 1), and an important addition has thus been made to the series of securely-dated Anglo-Saxon pottery of this area. Adjacent trenches were investigated, but no traces of any structures were revealed although the finding of over fifty fragments of Late Saxon pottery (all of Thetford ware) with fragments of lava querns and animal bones attested domestic occupation in the immediate vicinity in the 10th or 11th century.

THE MORLEY ST PETER HOARD

On 3 February Mr C. E. Blunt, the President of the Royal Numismatic Society, and Mr R. H. M. Dolley from the Department of Coins and Medals of the British Museum, together with Mr D. M. Metcalf of St John's College, Cambridge, visited Norwich to assist in the sorting and classification of the treasure, and some of their preliminary conclusions were included in the writer's evidence of identification given at the inquest held by the Norwich District Coroner (Mr G. W. Barnard) at Wymondham College on 10 February. A schedule of the coins was submitted, and the opinion expressed that the nature and conditions of finding clearly indicated the deliberate concealment of a very substantial sum of money, with intention of its eventual recovery, in or about the year 925. The jury accepted this argument, and returned a verdict of Treasure Trove, the two workmen being named joint finders. The Coroner then seized the coins in the name of the Crown, it being made clear that the finders would receive in due course the customary *ex gratia* reward of the full market value of the hoard. After being photographed at the Norwich Museum the coins were sent to the British Museum for further examination and eventual publication, and it is hoped that it will be possible for the whole hoard to be divided between the two institutions.

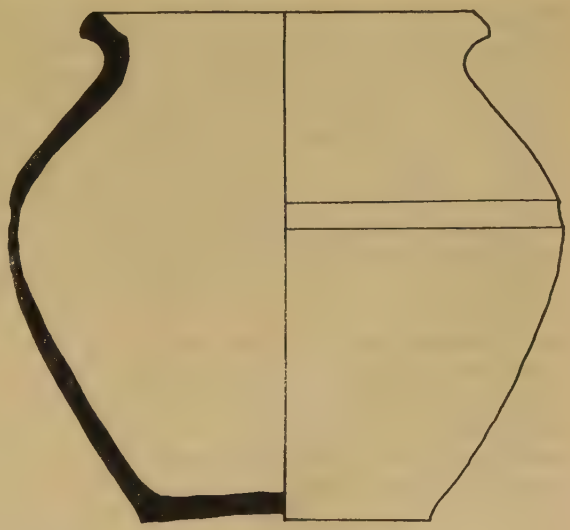


FIG. 1. Reconstructed jar containing the coin-hoard from Morley St Peter, Norfolk. (Half actual size.)

In the evidence submitted at the inquest, it was pointed out that the coins could be arranged in three chronological groupings. Group I comprises 107 coins ranging from *c.* 875 to *c.* 905. The earliest coin by far is one of Ceolwulf II of Mercia, and there were also three of Aethelstan II (Guthrum) of East Anglia struck in the penultimate decade of the 10th century, but the bulk consists of late coins of Alfred the Great struck almost entirely in south-eastern England in the decade immediately preceding his death in 899. Very few of the coins bear the name of a mint, but the signatures do occur of Canterbury, Exeter, London and Winchester. The group is rounded off by nineteen of the memorial coins of St Edmund struck by the Danish settlers in East Anglia, possibly at Norwich and/or Thetford, between *c.* 890 and *c.* 905 (or even later ?), and by half a dozen of the earliest coins of Edward the Elder, again with a very pronounced south-eastern flavour. Group II consisted of only sixteen coins, all apparently struck at York. Earliest is a penny of a mysterious ' Cnut ' who must have ruled *c.* 900—the old identification with Guthred

(d. 895) has recently been disproved on purely numismatic grounds—and there follows a run of thirteen of the early ('swordless') coins with the name of St Peter struck *c.* 905–915. The Northumbrian element ends with an extremely rare secular penny struck *c.* 915 and a late St Peter penny on both of which the type is a Viking sword.

Group III is by far the largest of the three groups and, with the exception of a solitary penny of Aethelstan (924–939), is composed entirely of late coins of Edward the Elder struck in the second half of his reign, the great mass of them after *c.* 920 and almost certainly at a mint or mints in East Anglia. It is this group which has attracted the attention of numismatists, and the patient study of its long chains of die-identities will throw light not only on the origins of the Norwich mint, but on wider problems of Anglo-Saxon mint-organization generally. It is fortunate indeed that the recovery of this intact treasure should have coincided with the emergence of a new school of Late Saxon numismatics equipped and anxious to extract from the find information that in the past might well have gone by default.

R. R. C.

(*b*) The Significance of the Find

Paradoxically the new hoard is almost as important for what it omits, and hence the emphasis laid on its recovery intact. The absence alike of Kufic dirhams and of Carolingian deniers is consistent with prompt application to the reconquered Danelaw of the principle 'ðaet gange an mynet ofer ealne þæs cyninges anweald'[1], and one may be well satisfied that there would seem to be no coins of the types and styles which recent papers have sought to associate with a mint or mints established by Alfred in north-western Mercia. The absence of the only less common coins with the name of Archbishop Plegmund which were struck at Canterbury *c.* 890–895 and again *c.* 910–914 underlines the paucity of coins in the new hoard which can be given to the period *c.* 900–915, and it is tempting to see in this a reflection of the inability of the Danes of East Anglia to plunder on West Saxon territory after 902. That Plegmund coins did in fact circulate pretty freely south of the Thames is implied by the as yet very largely unpublished hoard of 500 English coins which was concealed in Rome perhaps no more than a year after the deposit of the new hoard from Norfolk. The Rome find contained only a handful of those coins of Edward which the Morley St Peter treasure trove has now firmly linked with a mint in East Anglia, and even fewer of the coins associated with the Chester area, and inevitably the new hoard must renew speculation on whether or not Romescot was levied outside the West Saxon kingdom proper (Wessex, Kent and London) before the reign of Aethelstan.

As Mr Rainbird Clarke has pointed out, it is the late grouping of 757 coins of Edward the Elder that is of such extraordinary interest for the numismatist. Only 89 are without portrait, whereas the Romescot hoards suggest that in southern England portrait coins constituted only one in six of normal issues. Already numismatists were suspecting that East Anglia had a special predilection for the portrait—under Eadmund for example the comparatively rare portrait coins are predominantly of the Norwich mint—and the Morley St Peter find may be said to have provided final confirmation of this.

In the first place it is notable that there are almost no affinities between the 89 non-portrait coins and the great bulk of those with portrait. No more than 12 of the former appear to have been struck on or south of the Thames, and the balance have an epigraphy which is associated with Mercia east of Watling Street, i.e. with the area progressively occupied by the English before the final submission of East Anglia as such. Of the 668 portrait coins, 3 only would seem to have been struck in the West Saxon kingdom

[1] 'That one coinage is to be current throughout all the king's dominion.'

proper. The remainder, which include no fewer than 55 of the hitherto extremely rare variety with right-facing bust, form a definite unity, and it is impossible not to consider this coinage purely East Anglian.

First and foremost we must draw attention to the phenomenal degree of die-linking. As many as 25 coins have been noted from a single pair of dies, and on the accompanying plate (PLATE XI) an attempt has been made to give an impression of some of the ways in which a comparatively small number of dies are used in a wide number of combinations. The top row (nos. 1–5) shows the same obverse die combined with no fewer than five reverses, three purporting to bear the names of moneyers ' Deorwald ', ' Heremod ' and ' Igeret ', and two completely blundered. Nos. 6–13 show ' Liofhelm ' sharing an obverse die with ' Diermod ' who shares with ' Gareard ' who shares with ' Aethelfreth ' who shares with ' Liofhelm ', and it must be emphasized that these chains of die-links can be multiplied almost indefinitely. Nos. 17 and 18 afford an example of the same sort of link among the right-facing coins, while nos. 14–16 show the same reverse die combined with no fewer than three obverses, a piece of mint-practice that it would be hard to parallel in a later period. Mr C. E. Blunt expressed the guiding principle extremely succinctly when dealing with the little group of seventh-century thrymsas very improbably associated with Bishop Mellitus, and his words are even more apposite in the present context—' all these coins that are so strongly die-linked can only recently have left their place of issue '. While, therefore, the numismatist may have his reservations as to whether the coins may necessarily have been struck by or even for the men whose names appear in more or less blundered form upon them, he cannot but be confident that they represent a cross-section of the output of a major mint established in East Anglia at the close of the reign of Edward the Elder. The political implications of this will not be lost upon the historian, and it is further suggested that all the evidence points to the mint having been at Norwich. Thetford seems the only conceivable rival for the honour, but no mint-signed coins of Thetford are known earlier than Eadgar whereas mint-signed coins of Norwich of Aethelstan are by no means uncommon.

R. H. M. D.

The Excavation of Two Long Barrows in Wessex

by FAITH DE M. MORGAN AND P. ASHBEE

This article, by two of the archaeological consultants to the Ministry of Works, gives interim reports on the excavation of two long barrows in Wessex. The first is the Nutbane Long Barrow near Andover excavated by Miss Faith de Malet Morgan; and the second, the Fussell's Long Barrow, not far from Figsbury Rings (discovered by the late Editor of ANTIQUITY *on one of the sorties flown for* Wessex from the Air) *is by Paul Ashbee. The Fussell's Lodge excavation was entirely sponsored by the Ministry of Works: the Nutbane excavations were organized by the Andover History Group with a grant from the Ministry.*

1. NUTBANE LONG BARROW

by FAITH DE M. MORGAN

THE long barrow at Nutbane, Penton Grafton, Andover, Hants, was excavated during the autumn of 1957 by the writer on behalf of the Andover History Group. The barrow (map reference SU 330495) was not included in the map of *Neolithic Wessex* and had only been discovered two or three years previously, when the attention of Ordnance Survey officers was drawn to its presence.

The barrow had been considerably denuded by ploughing over many years. It is built along the contour of a gentle slope, part of the gradual incline from the chalk ridge five miles to the north down to the valley of the Anton River two miles to the south. It is orientated East–West, with the higher and broader end to the east.

Excavation revealed that the wedge-shaped mound was 170 ft. long and 75 ft. broad at its east and widest end, narrowing to approximately 25 ft. at the west end. Between this and the flanking ditches, which followed the lines of the mound, there was a berm 30 ft. wide. The north ditch was 180 ft. long and the south 200 ft. long and both extended well beyond the west end of the mound; their width varied between 15–18 ft. and their depth between 8–10 ft.

The character of the silting in the ditches did not vary in any of the sections excavated. A few sherds of Windmill Hill pottery were found in the primary chalk silting; above the layers of slow silting and nearly level with the edges of the ditches, a layer of dark soil marked the Beaker horizon in which was found part of a B beaker; and a few sherds of Middle and Late Bronze Age pottery were stratified above this layer, but below the well-marked Romano-British horizon.

At the east end of the barrow two structures were found; a mortuary enclosure of post and log construction 20 × 18 ft., which contained four burials, and immediately to the east of this the foundation trenches of a forecourt enclosure or building 40 × 20 ft. These structures represent five phases of building, apart from the raising of the mound which was itself subdivided, and it will be best to describe these in their chronological sequence, which is as follows:

1. First forecourt building.
2. Banked enclosure with entrance posts, and three burials.
3. Second forecourt building.
4. Mortuary enclosure fence built, replacing banked enclosure, one burial, cairn covering all four.

PLATE IX

(*a*) Early stadium showing base of starting gates

See p. 87]

(*c*) Excavated section of the Diolkos from the East

See p. 80] [*Photo:* N. M. Verdelis

(*b*) Starting gates, reconstructed. The author is standing in the pit, manipulating the cords

See p. 87]

(*d*) Isthmian wall built by Justinian

See p. 83]

(*a*) Wansdyke on the Marlborough Downs, looking west. Morgan's Hill is on the horizon

(*b*) One of Skinner's two sections across the Roman road, Neston Park, drawn 1819

See p. 95]

[*Photo:* British Museum

PLATE XI

Saxon coins from the Morley St Peter hoard

[*See p.* 103

PLATE XII

Nutbane Long Barrow: oblique air photograph of eastern end during excavation

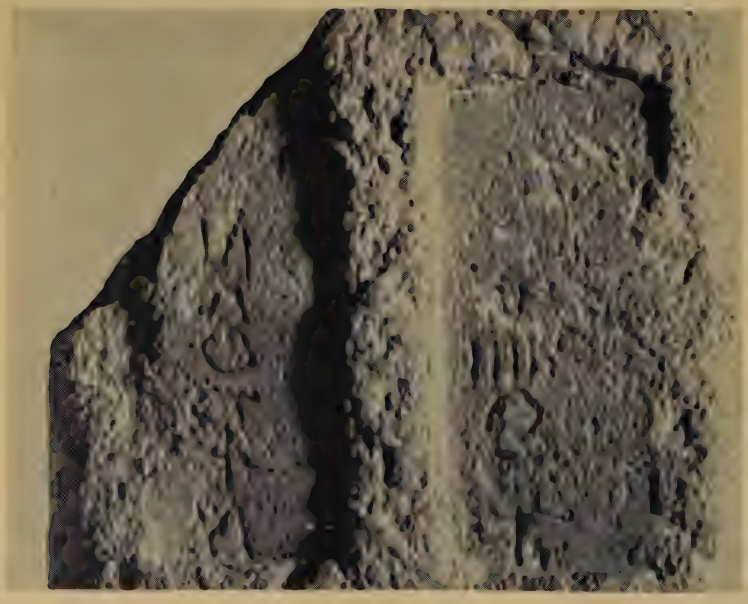
[*See p.* 106

Eastern view of the Fussell's Lodge long barrow from the air

[*See p.* 108

(*a*) Fallen column from tomb South C

[*See p.* 114 and Fig. 2



(*b*) Altar from a Romano-Berber shrine

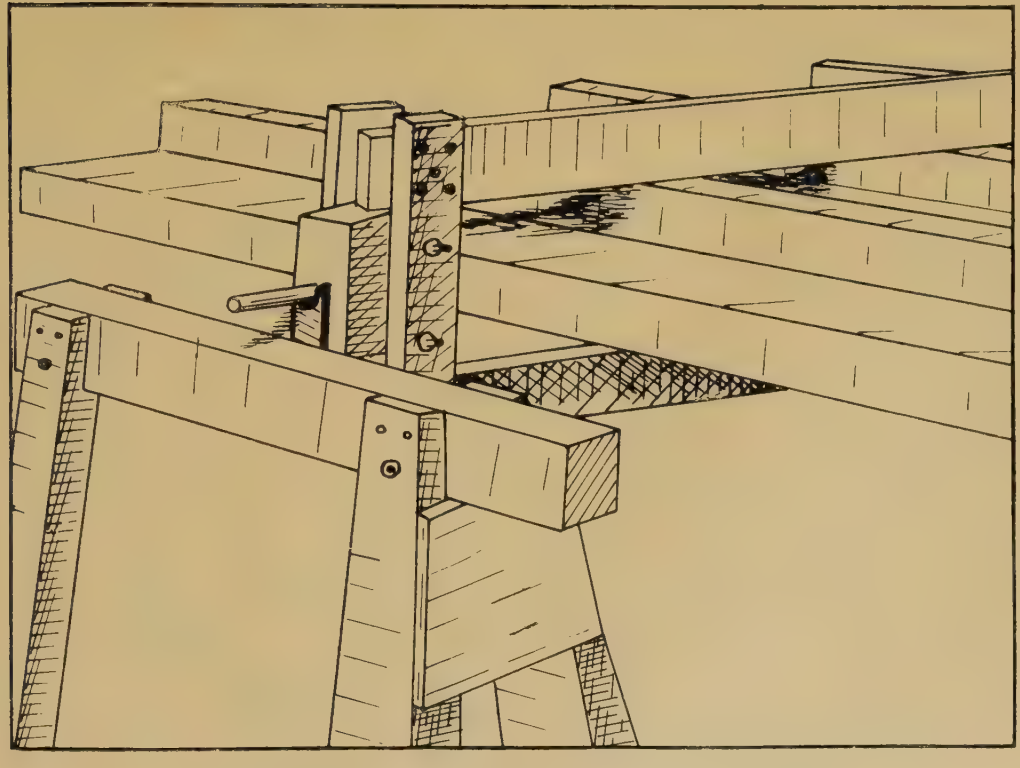
See p. 114 and Fig. 3]

(*c*) Two columns of graffiti from a Romano-Berber shrine

See p. 115 and Fig.

(*a*) Undercutting and rolling back the mosaic attached to a sheet

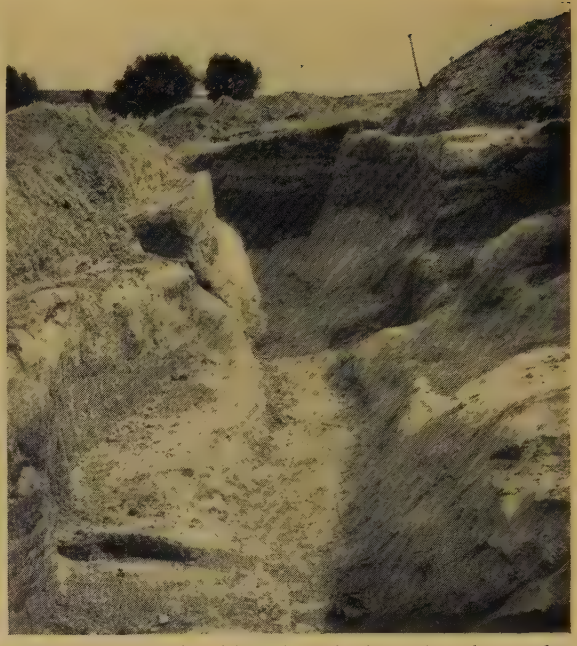
[*See p.* 117



(*b*) Sketch to illustrate method of rotating framework

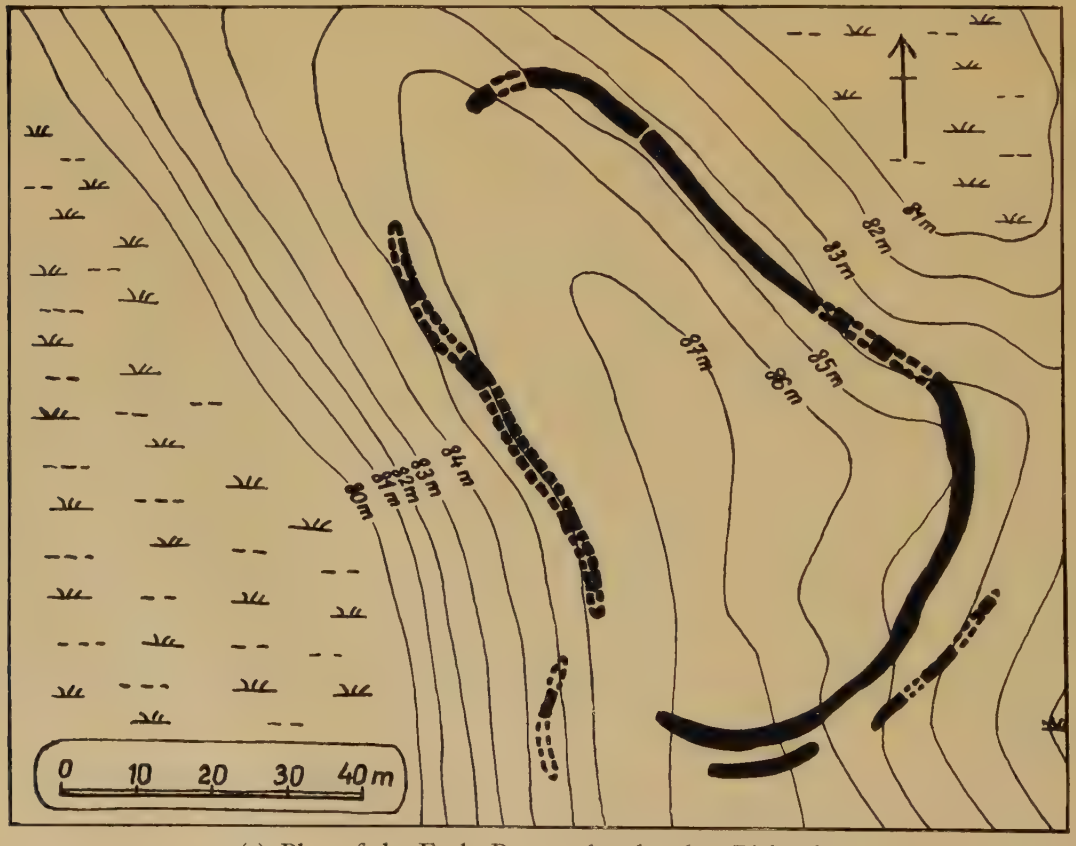
[*See p.* 119

(*a*) Biskupin. Main trench in S. part of kraal. Dots mark later deposits, dashes the ditch itself



(*b*) Main trench of kraal with deposits above the ditch. Eastern part viewed from North

See p. 122]



(*c*) Plan of the Early Bronze Age kraal at Biskupin

See p. 121]

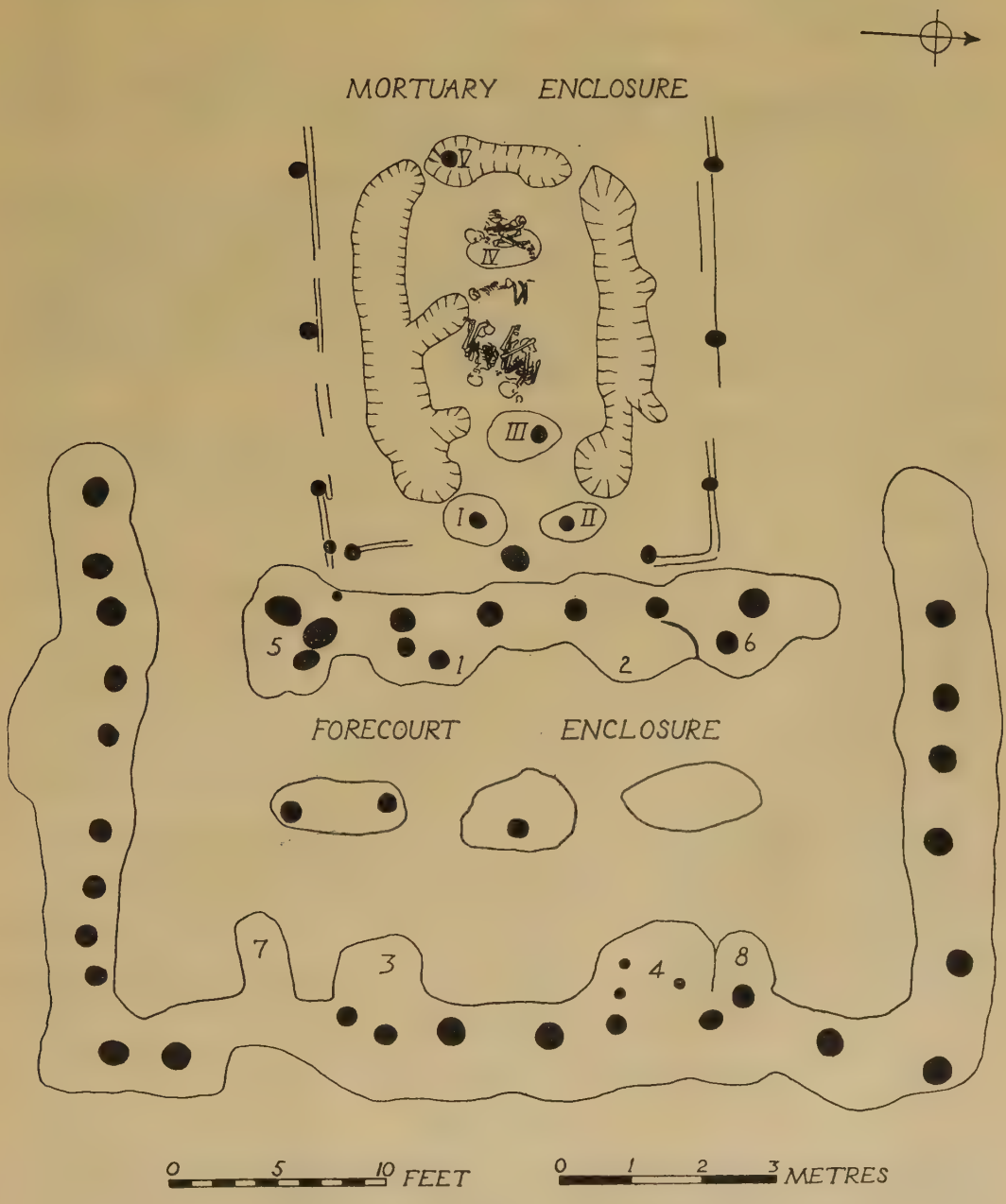

NUTBANE LONG BARROW
MORTUARY ENCLOSURE
V
IV
III
I
II
5
1
2
6
FORECOURT ENCLOSURE
7
3
4
8
0 5 10 FEET
0 1 2 3 METRES

FIG. 1.

5. Post and log fence across east end of mortuary enclosure
6*a*. Mortuary enclosure filled with soil from flanking ditches, primary mounding round this and forecourt.
6*b*. Forecourt burnt.
6*c*. Secondary mounding over all. (PLATE XII.)

The first forecourt was a small building 16 × 14 ft., its greater measurement aligned E.–W. This was supported by three or four posts, 6 in. diameter, in each of four large postholes 3–5 ft. across and 5 ft. deep (1–4 on plan).

At probably the same time, a rectangular banked enclosure was built to contain the burials, immediately to the west of the forecourt building. The bank was thrown up outside the shallow irregular ditches from which the material for it was obtained. At the east end of this enclosure there were two posts forming an entrance (I and II on plan), and at the head and foot of the burial area there was a standing post (III and IV). The material from these post-holes, and probably the surplus from those of the forecourt, increased the height of the bank, especially on the north side where the ditch was shallower. Three crouched and articulated burials were placed between the head and foot posts, laid on brushwood, and covered with soil.

The first forecourt building was destroyed before the second forecourt was built on a larger scale and aligned acrosss the barrow, but still with an entrance east and west. A discontinuous rectangle of post-trenches 4 ft. wide and 4 ft. 6 in. deep was dug, cutting through the original holes, which contained posts 1 ft. in diameter and 2–4 ft. apart. Three holes down the centre held posts to support the roof. The bays 5, 6, 7 and 8 held the corner posts of the building and the posts in the north and south trenches were free-standing, forming a colonnade or façade which returned on the east side as far as the NE. and SE. corners of the building.

Subsequently the banks of the burial enclosure were pulled back into the ditches and levelled, and the fenced enclosure, of posts supporting horizontal logs, was built where the banks had been. The foot post, IV, was removed, and the last burial placed over the hole in a crouched position. A cairn of chalk was piled over all four burials.

A ' blocking ' was then effected by the erection of a post and log fence across the east end and entrance of the enclosure, which was filled with top-soil from the flanking quarry ditches, begun at this stage to provide the material for the mound.

Mixed chalk and soil from this source was piled against the mortuary enclosure fences and through the façade up to the walls of the forecourt building. This building was then partially fired, causing the posts to smoulder down into their sockets.

Finally all the structures were covered with the coarse chalk from low in the ditches, and the mound was completed.

A few sherds of Windmill pottery were found on the old turf line, in the mound material, and in the forecourt post-trenches where there were also a few fragments of antler picks.

2. THE FUSSELL'S LODGE LONG BARROW

by PAUL ASHBEE

ON 14 July, 1924, during one of the now historic sorties flown for the compilation of *Wessex from the Air* (1), the late Dr O. G. S. Crawford observed a long barrow almost midway between Fussell's Lodge Farm and Figsbury Rings (2). Two days later he visited it, recorded its length as 171 ft., its height as 5 ft. 4 in., and noted many

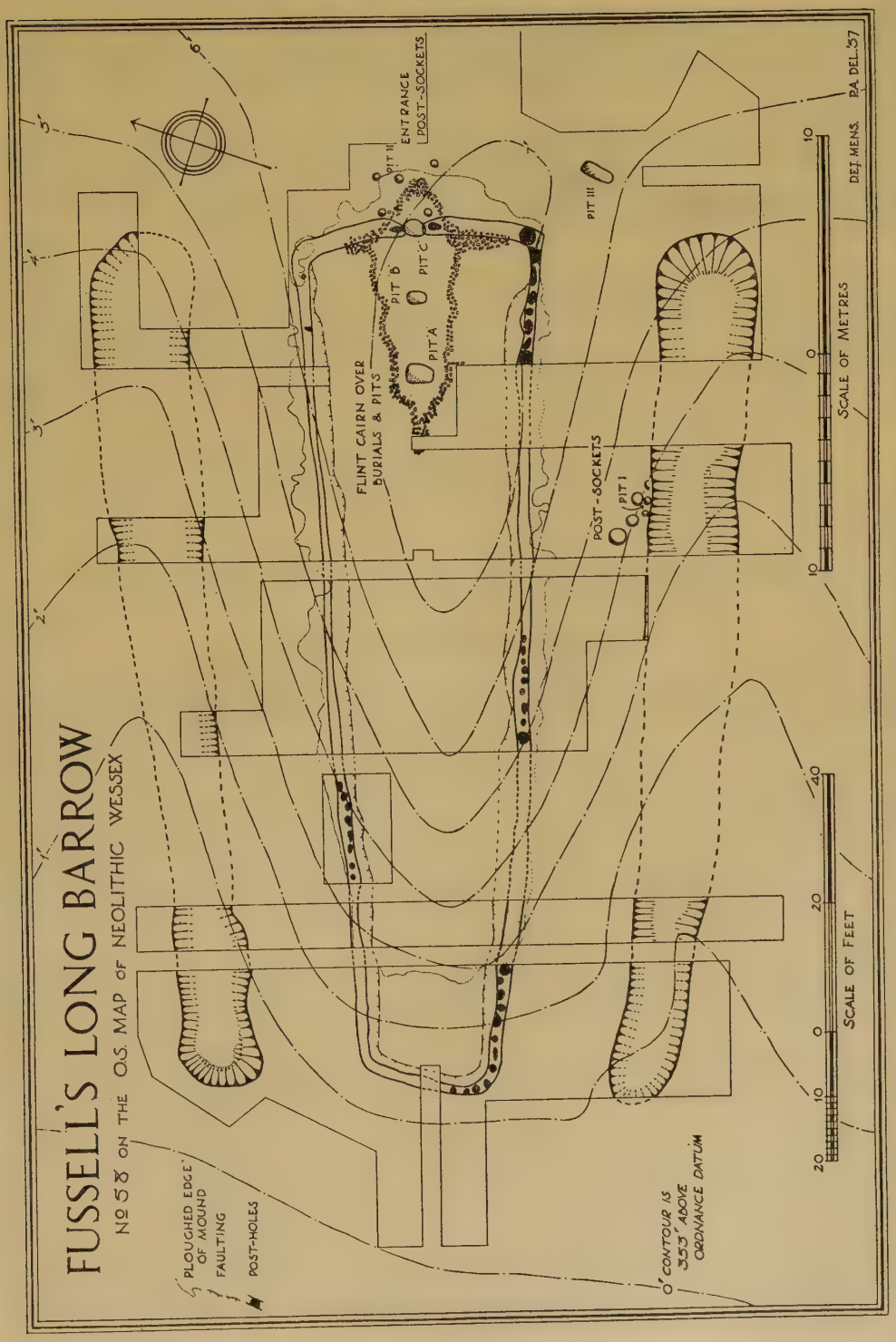


FIG. 2. Plan of the barrow.

large flints over the higher, eastern end. At that time it appeared that the field in which it stood had been under plough at no distant date. Subsequently, a notice of this long barrow was incorporated in the *Map of Neolithic Wessex* (3) as ' No. 58, the Fussell's Lodge Farm Long Barrow '.

Excavation by the writer (FIG. 1) was undertaken in 1957 on behalf of the Ministry of Works in advance of a land improvement scheme. Hampered by bad weather, the operations occupied some ten weeks (4), and though no more than the dimensions of the ditches were established, it was found possible to excavate the mound in near totality.

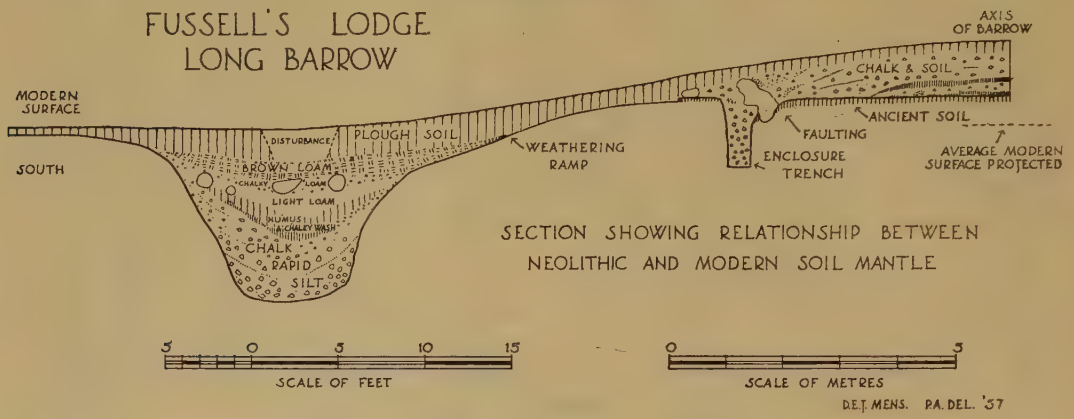


FIG. 3. Half transverse section of the barrow.

The ancient soil beneath the barrow was black, and contained numerous worm casts. Dr I. W. Cornwall (who examined this soil) considered it a buried rendzina. Stones throughout the profile suggest some mechanical disturbance before the building of the barrow, and the fact that rendzina characteristics were present in spite of disturbance weighs against prolonged cultivation. Possible evidence of the nature of this disturbance was recovered, from the whole area beneath the barrow, in the form of parallel ribs of chalky marl showing distinctly against the darker soil and lying diagonally athwart the axis of the barrow. It is thought that these *might* have been the result of a single act of hoeing in rows. A single act because, had this not been the case, the ribs would scarcely have survived.

Differential weathering accounts for nearly 2 ft. difference in physical height between the surface of the ancient soil sealed beneath the barrow and the present average surface about the barrow (Fig. 3). This phenomenon has been recently commented upon in ANTIQUITY (5).

Under the denuded barrow mound was found the palisade trench of a timber-built enclosure, 140 ft. in length, and trapeze-shaped in plan (PLATE XIII), 20 ft. in width at the narrow end and 40 ft. at the broad end. It had been constructed by digging a vertically-sided trench about 1 ft. 6 in. wide to a depth of about 5 ft. below the ancient surface at the sides and around the narrower end, and of about the same depth although more than twice as wide at the broader end. In the middle of the more massive, broader, end, a causeway had been a means of access to the interior of the enclosure. The trench had held vertical timbers which had originally stood as an enclosure and later appear to have revetted and retained the barrow. The dimensions of many of these long decayed timbers were obtained by the careful excavation of soft replacement material from the holes left in the firm

packing. Many of these timbers at the sides and narrower end of the trapeze appeared to have been trunks, some 1 ft. 2 in. in diameter, while others appear to have been split trunks of commensurate dimensions. They had been set up from 1 ft. 6 in. to 2 ft. apart. At the corners of the broad end of the trapeze, there had been set massive circular trunks, almost 3 ft. in diameter, while trunks of nearly the same size had been set along it. Four post sockets outside the entrance to the enclosure suggest there may have been a porch.

It would seem almost certain that horizontal timbers, or wattles, had aided this vertical palisading in the retention of tipped soil and chalk. However, no trace remained, owing to the slide and settlement of the chalk mound.

The record of timbers in the trench was fragmentary because as they decayed, the inner lip, which bore the weight of the mound, faulted under this pressure. Thus, in places where this pressure was considerable, the trench was collapsed and distorted, its edges being almost closed. It is not surprising, therefore, that the character of the trench did not always permit of the detection of the erstwhile vertical timbers.

Such vertical timbers, as the evidence summarized above suggests, set to such a depth in the chalk, could have stood to a height of some 7 ft. 6 in. to 9 ft.

Within the trapezoidal enclosure, and in the area subsequently to be occupied by the burials and their covering cairn, two pits (A and B FIG. 1) were dug into the ancient soil, on the main axis. Their filling was found to be dirty chalk and weathered flint nodules. The furthermost pit (A) from the enclosure entrance was but half infilled when pottery and bones were put in position. Stacked long bones protruded over its inner edge and pieces of the near intact pot crushed by the weight of these bones, and the superincumbent cairn, fell into it, to be found as the infill was removed, and subsequently joined. Cairn construction completed the infill of this pit. Completely beneath the burials was the smaller pit (B), filled to within 9 in. of its lips, the upper space being filled almost entirely with broken bones. In the flint nodule and chalk infill were pieces of burnt and weathered bone and charcoal fragments.

The burials had been laid upon the ground surface and, cleared of the cairn, they were an impressive sight. A great wedge-shaped mass of bones extended from the enclosure entrance to, and over, the edge of the furthermost axial pit. Five distinct groups comprised this burial complex. Immediately within the enclosure entrance was an articulated contracted burial, at the heels of which was a much broken bovine skull and at the knees a small broken pot. Next to it was an articulated arm and leg, with by them a skull. Between this phenomenon and the two main stacks of disarticulate bones were skulls and much decayed fragmentary long bones. The skulls of the first of the main stacks were at the top while the long bones were, for the great part, set *across* the axis of the complex. Between the two main bone stacks were child burials. The long bones of the furthermost stack were stacked *along* the axis of the complex, and the ends of many of these protruded over the inner lip of the furthermost pit. Skulls were at the sides which were retained by flint nodules. Under this stack were the greater parts of two carinated pots, some of which had fallen into the pit, as was related above.

Many of the bones and skulls were in an advanced state of decay, fragmentary, or broken, and in such bone stacks the identification of individuals, under field conditions, was a matter of some difficulty. However, it would seem likely that here are the remains of some fifteen to nineteen adults and children. The varying degrees of ossification of the skull sutures suggest a disparity of ages.

The greater part of the interior of the enclosure was cleared, no bone being found except within this the burial complex, suggesting that no deposition of bodies had taken place

except within this restricted area. If it is found that parts of individuals are missing it would seem probable that exposure was undertaken elsewhere and that the enclosure was erected for the funeral rites of the complex stacked within it.

Over the burials was a cairn of weathered flint nodules. This was wedge-shaped, the wider end being immediately inside the enclosure entrance and aligned along its axis, and the distal end covering the furthermost pit and continuing over the enclosure entrance. Amongst the material of the cairn was the waste of flint knapping, in the form of discarded cores, flakes, splinters and spalls, and a number of axe rough-outs. Before the cairn was built, turf, stripped presumably from the site of the ditch, had been tipped upon either side of the burials, the cairn springing from this. Although numbers of flints had been dragged out by the plough a thin skin of chalk remained, showing that in great part the cairn remained at its original height. From the area above the innermost bone-heap the partially disturbed scattered, weathered, and apparently broken bone fragments of an adult were recovered, while at about the central point on the axis a neat pile of bovine metacarpals and metatarsals were found.

At some stage either upon the completion of the cairn, or upon the infill of the enclosure, a pit was dug across the causeway which had given access to the enclosure interior. This had been infilled with weathered flints which were part of the entrance blocking. In this pit and blocking, of which only a lobe much damaged by ploughing remained, was a mass of yellow burned chalk, fire crackled flints and large pieces of charcoal. The remnant of the entrance blocking, which had subsided, covered one of the post-sockets of what may well have been a porch, within the area of which was a small circular flint and sherd filled pit.

The basis of the infill of the enclosure was a series of heaps of turf tipped together with the yellow weathered chalky material which was immediately beneath the ancient soil mantle. Immediately beyond the cairn they were upon either side of the axis, one tip avoiding what was perhaps a natural tree-hole. At the narrower end of the enclosure the tip was broad and axial. In one or two places straight edges showed where this material had been tipped against the revetment provided by the enclosure posts.

A series of post-sockets from which posts had been withdrawn were found on the inner edge of the southern ditch. Their position suggests that they could have supported a platform to which chalk was hauled up from the depths of the ditch. On their removal a small circular pit seems to have been dug, almost identical with that found in the porch, but in this instance containing charcoal.

It was not possible to excavate more than representative parts of the flanking ditches on account of their magnitude. However, it could be clearly seen that the breadth and depth of each ditch was greatest at the broader end of the trapeze. Silting was rapid, and the basal accumulation of rubble and rain-wash may not represent more than a few years (6). In the rapid silt were pieces of shed antler and nests of flakes from knapping. These point to subsequent visits to the barrow by its builders, this being suggested also by a heap of articulate bones representing the remains of joints of ox-meat.

From the initial accumulation of humus in the ditch came a number of sherds of Rusticated Ware (7), and dark-faced Windmill Hill sherds. Apart from the pottery with the burials and stray plough soil sherds nothing more was met with in the barrow. There was no beaker pottery in the upper silts (8).

The affinities of the plan of the Fussell's Lodge Long Barrow lie, in the British Isles, with the trapezoid plans of certain Severn-Cotswold Chambered long barrows (9), and of members of the Clyde-Carlingford group, as well as in the wedge-cairns of Ireland (10). The slightly convex broad end of the enclosure could be likened to the similarly convex façade of the West Kennet Long Barrow, which differs from the cuspidal forecourt so

often a feature of the series. The mode of construction of the enclosure was that of the rectangular structure detected beneath and in the Wor Barrow (11), while 'ritual ' pits have been frequently found under long barrows and cairns (12).

An enclosure recognized only recently at Dorchester, in Oxfordshire (13), and an apparently comparable example, badly plough damaged, on Normanton Down, in Wiltshire (14), both appear to be like the pilot-ditched enclosure which preceded the main ditch of the Wor Barrow. If this is the case this type of monument would appear to be allied to the Dorset type of long barrow (15), rather than to that of Salisbury Plain.

Further afield, affinities with long barrows, embodying trapezoid features and, indeed, with long houses of trapeze plan on the north European plain as far east as Poland can be cited. Although for many years the ragged remains of the Medway Chambered Tombs (16) have been compared with the Hun's Beds of Holland and beyond (17), it is only recently that, in default of satisfactory Western progenitors (18) for our long barrows and for aspects of our Windmill Hill culture (19), we have turned to Northern Europe where more comparable material may exist. The excavation of Fussell's Barrow has contributed another important piece of knowledge from the British end.

References

(1) *Wessex from the Air*, 31.
(2) *Op. cit.*, 84–6.
(3) *Map of Neolithic Wessex*, Ordnance Survey, 1932.
(4) In the field my principal helpers were D. Johnston, G. Connah, D. Clarke, E. and D. Appleby, and T. Miles. They and many student and other assistants contributed materially to the work; without them it would have been impossible. Only numbers compel anonymity.
(5) Atkinson, *Stonehenge*, 10; *Antiquity*, xxxi (1957), 219–233.
(6) Pitt-Rivers, *Excavations*, IV (1898), 24; *Antiquity*, IV (1930), 97–100.
(7) *P.P.S.*, II, 19–23; Piggott, *Neolithic Cultures*, 341.
(8) *Proc. Bristol Univ. Spel. Soc.*, V, 25–43.
(9) Crawford, *Long Barrows of the Cotswolds*; *P.P.S.*, V (1939), 139.
(10) Childe, *Prehistoric Communities of the British Isles*, 55; Piggott, *Neolithic Cultures*, 159.
(11) Pitt-Rivers, *op. cit.*
(12) *Arch.*, LXXXV, 87–89; *Arch. Camb.*, C, 17.
(13) *A.N.L.*, IV, 56–9 (Site VIII).
(14) Piggott, *Neolithic Cultures*, 59.
(15) Crawford, *Wessex from the Air*, Pl. XV; *P.P.S.*, I (1935), 125–6.
(16) Crawford, *O.S. Prof. Paper, N.S., No.* 8., Jessup, *Arch.Kent*, 61–87.
(17) Ward, *Arch. Camb.*, 6th Series, XVI, 242, initially put forward this view.
(18) *Antiquity*, XI (1937), 441
(19) *P.P.S.*, XXI, 96–101.

Inscriptions in the Libyan Alphabet from Ghirza in Tripolitania

by J. M. REYNOLDS, OLWEN BROGAN and DAVID SMITH

In this article Miss J. M. Reynolds, Fellow and Director of Studies in Classics at Newnham College, Cambridge, Mrs Brogan, Secretary to the Faculty of Archæology, History and Letters of the British School at Rome and Mr David Smith, Curator of the Museum of Antiquities, King's College, Newcastle-upon-Tyne, describe some unique inscriptions which they have recently found in North Africa.

GHIRZA is the site of a Roman frontier settlement in the pre-desert zone of Tripolitania in North Africa, 5 miles south-west of the confluence of the Wadi Ghirza and the Wadi Zemzem, 80 miles from the coast of the Gulf of Sidra and 150 miles (in a straight line) south-east of Tripoli. It is best known for its two groups of monumental tombs decorated with rough sculptured reliefs showing the life of the inhabitants; but it is also notable for other antiquities, and especially for its ' town ' of fortified farmhouses, clustered together on the edge of the wadi.[1]

Several inscriptions in Latin characters have been found here in the past, all on or beside the northern group of tombs close to the settlement. Some of these show a passable knowledge of the Latin language, and two of them record the cost of building tombs in terms of *folles*,[2] which shows that these at least cannot be earlier than the 4th century A.D. Three others are in a language almost entirely unintelligible, no doubt the native Libyan of the time and an early form of Berber.[3] Similar inscriptions have been found elsewhere in Tripolitania and have been labelled Latino-Libyan; occasionally they contain intrusive letters from a non-Latin alphabet to express sounds for which Latin characters did not cater; they seem, where dateable, to be at earliest of the 4th century A.D. and sometimes later.[4]

To these documents must now be added a group of inscriptions written entirely in a non-Latin alphabet, copied by the present writers between 1953 and 1957 in the buildings of the Ghirza settlement and from the tombs. The alphabet used seems to be a version of the Libyan alphabet already known from Tunisia, Algeria and Morocco.

This Libyan alphabet is first found in the territory of the native (i.e. Berber) kingdom of Numidia ruled by Massinissa and his descendants after Rome's defeat of Hannibal and the Carthaginians. It survived not only the full Roman occupation of North Africa, as is shown by a number of bilingual texts in Libyan and Latin, but also the Arab invasions,

[1] For the tombs see Olwen Brogan, *Illustrated London News*, 22 and 29 January, 1953; D. E. L. Haynes, *The Antiquities of Tripolitania* (1955), pp. 154 ff.; for the settlement, *Illustrated London News*, *loc. cit.*; Olwen Brogan and David Smith, ' The Roman Frontier Settlement at Ghirza: an interim report ', *Journal of Roman Studies*, XLVII (1957), 173–84.

[2] *Folles* are silvered bronze coins first introduced by the Emperor Diocletian (A.D. 284–305).

[3] J. M. Reynolds and J. B. Ward Perkins, *Inscriptions of Roman Tripolitania* (1952), pp. 222 ff., nos. 898–90 in Latin and nos. 901 and 903 in Latino-Libyan; see also J. M. Reynolds, ' Inscriptions of Roman Tripolitania: a supplement ', *Papers of the British School at Rome*, XXII (1955), pp. 135 ff.

[4] R. G. Goodchild, ' The Latino-Libyan Inscriptions of Tripolitania ', *The Antiquaries Journal*, XXX (1950), 135–44.

for in a developed form, called *Tefinagh*, it is still used to-day by the Tuareg of the Sahara.[5] The earliest known instance—and incidentally the only dated text that has been found—is of 139 B.C. and comes from Dougga in Tunisia.[6] It is one of a small group, all no doubt approximately of the same time, and nearly all bilingual in Libyan and Punic, that record the erection of public buildings at Dougga and are cut with some pretensions to elegance, clearly on the model of public building inscriptions in Carthage and other Punic towns. These, like the Punic texts, are written in horizontal lines, and read from right to left.[7] But the eleven texts of this group are unique. The remainder of some 1,100 Libyan inscriptions recently collected by Monsieur J.-B. Chabot, are on tombstones from country districts, simple and brief, and, as a general rule, roughly cut by rustic workmen. Their letters are written in vertical columns—both Punic and Latin traditions being here abandoned—to be read in most cases from the bottom upwards, starting sometimes from the right-hand, sometimes from the left-hand column, apparently according to the whim of the workman. In the greater number of texts words are not distinguished from one another, although occasionally dots are used for this purpose.[8]

Chabot's inscriptions are all from Tunisia, Algeria or Morocco; he has rejected, after enquiry, all examples reported in the past from sites east of the Tunisian/Tripolitanian frontier. They range in date from the 2nd century B.C. well into the period of the Roman Empire; but he does not think that he can point to any that are certainly later than the 3rd century A.D. As has been said above, they are, in overwhelming majority, simple epitaphs.

These facts give a special interest to the new texts from Ghirza. Here are some clear examples from Tripolitania. One of them is unlikely to be earlier than the 4th century A.D. and most of them are probably much later, to judge from their archaeological contexts[9]; so that they help to fill the gap between the ancient and modern usage of the Libyan alphabet. Moreover, the majority of them come from buildings and cult-objects in the settlement; only two were found on tombs and these may well not be funerary.[10] If they could be understood, they would probably throw light on the everyday life and beliefs of the ancient Libyans in a way that simple epitaphs cannot do, as well as on the history of Ghirza itself after the withdrawal of Rome. They are all quite short. They are normally written, like the majority of Chabot's texts, in vertical columns, presumably to be read from the bottom upwards. But their transliteration and interpretation present problems that have not yet been solved.

Chabot divides his texts into two groups, an Eastern and a Western, although he does not wish to insist too rigidly on the geographical implications of these terms.[11] The Eastern group includes a number of bilinguals in Libyan and Punic and in Libyan and Latin, with the help of which French scholars have been able to establish, more or less

[5] J.-B. Chabot, *Recueil des Inscriptions Libyques* (Paris, 1940), Preface, p. i; J. Duveyrier, *Les Touareg du Nord* (1864), 386–90; Oric Bates, *The Eastern Libyans* (1914), 84–90.

[6] Chabot, *op. cit.*, no. 2; Valerius Maximus, I. 1. 21, Ext. 2, refers to an inscription cut for Massinissa himself on some elephant tusks, *gentis suae litteris*, but this has been thought to be in the Neo-Punic alphabet of Carthage.

[7] *Ibid.*, nos. 1–11.

[8] *Ibid.*, preface, pp. iv ff.

[9] No. 1 below is cut on a column of one of the monumental tombs in the northern cemetery (Tomb North A). This tomb cannot be dated exactly, but the two adjacent tombs, one of which appears to belong to the same family, have Latin inscriptions which mention *folles* (see note 2 above). We believe the Libyan inscription to be unconnected with the original owner of the tomb and therefore considerably later than its original construction, though it might be a graffito of one of the workmen.

[10] Nos 1 and 2 below.

[11] *Op. cit.*, preface, pp. iv ff.

securely, the value of the twenty-two alphabetic symbols used and to read a large number of Libyan personal names and a small number of Libyan words. (As the number of words used in the inscriptions is very limited, progress in the construction of a Libyan vocabulary is inevitably slow.) In the Western group there are no bilinguals; some additional symbols are used to the value of which there is, he thinks, no adequate clue; and he also believes that the value given to some symbols shared with the Eastern group may be different. In his view, therefore, inscriptions of this group cannot be transliterated at all at present. In it, or in an equally unintelligible group, he seems to place fourteen texts from southern Tunisia, some of them from the region of Gabes and Medenine, that is to say, from an area geographically within easy reach of Tripolitania.

For historical and geographical reasons southern Tripolitania has always had close connexions with southern Tunisia and the areas further west. All were part of the less civilized interior of Massinissa's kingdom and, under Rome, of the military zone called Numidia, clearly distinguished from the pacified and urbanized Africa Vetus. It appears to us possible that the continuance of these connexions in late antiquity and the early Middle Ages is illustrated in the Libyan inscriptions of Ghirza; for some of these contain a number of symbols that do not occur in Chabot's Eastern alphabet but that can be paralleled in his Western texts, including among these the small group from southern Tunisia mentioned above.

The Ghirza inscriptions may be grouped as follows:

(i) Two on columns of Roman mausolea.

(ii) Five on small altars and other objects found in Building 32, a late Romano-Berber shrine.

(iii) Three or more columns of graffiti on the facing plaster round a window built into the same shrine at a late period in its history.

(iv) Thirteen on door-jambs, lintels and elsewhere in the domestic buildings of the settlement.

As examples we reproduce here the two inscriptions of Group (i), and one each from Groups (ii) and (iii).[12]

1. (FIG. 1) Inscribed vertically on the north-west corner column of the tomb North A, a mausoleum in the form of a small Doric temple which has a Latin inscription above its door (see footnote 8 and Inscriptions of Roman Tripolitania, no. 899); the letters are very lightly incised and unobtrusive, and may easily have been cut long after the building was erected.

Height of text, *c.* 22 cm.

The symbols numbered 1, 2, 3 and 4 do not appear in either Chabot's Eastern or his Western alphabets. According to Charles Tissot (*Géographie comparée de la province romaine d'Afrique* (1884), vol. I, p. 518) the Tefinagh alphabet contains the symbol numbered 3.

2. (FIG. 2; PLATE XIV, *a*.) Inscribed vertically on a column found fallen from the tomb South C, a tomb in the form of a small temple surrounded by an arcade. The letters, which are of unusually large size for the Ghirza inscriptions, may have been cut after the fall of the column; but they are in any case of some antiquity since they are much more weathered than the other marks, known to be relatively modern, beside them.

Height of text, 1·30 m. Letters, ave. 0·03.

The symbols numbered 1, 2 and 3 do not appear in Chabot's Eastern alphabet; no. 3 appears in his Western alphabet; according to Tissot (*loc. cit.*) no. 1 occurs in the Tefinagh.

3. (FIG. 3; PLATE XIV, *b*.) Inscribed vertically on the front face of a rectangular altar, each column within a recessed panel. The altar, 22 cm. high, was found in the debris

[12] It is hoped to publish the inscriptions in full in *Papers of the British School at Rome*.

of Building 32, a Romano-Berber shrine, and is now in Tripoli Castle.

Height of text: (*a*) 0·06; (*b*) 0·05. Letters: 0·01.

Although the letters seem clear enough on the stone, they are so roughly cut that it is not easy to be sure in all cases what form was intended. Two further columns, almost illegible, are cut in similar panels in the back of the altar.

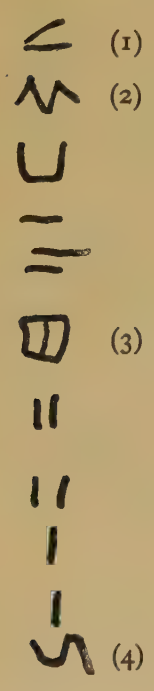


FIG. 1.

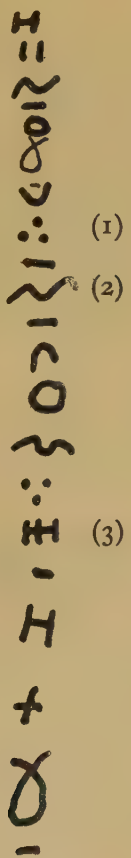


FIG. 2.

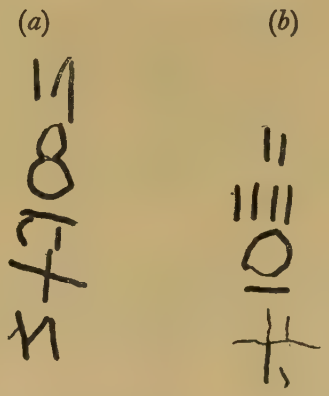


FIG. 3.

FIG. 4.

FIGS. 1–4. Examples of the Ghirza inscriptions, described in the text (pp. 114–115.)

4. (FIG. 4; PLATE XIV, *c*.) Two columns of graffiti from among the many incised in the plaster of a late wall in Building 32, a Romano-Berber shrine.

Height of the longer column: 0·24.

A number of graffiti have been cut alongside or even overlying one another; this, with the damaged condition of the plaster itself, makes it difficult to be sure that the marks transcribed above are correct in all cases.

Notes and News

LIFTING MOSAICS

PLATE XV

The discovery of a mosaic floor during an excavation always raises a problem of policy. Shall it be preserved *in situ*, removed to a museum, or reburied? Or shall it be photographed and destroyed?

To obtain the best results from the excavation, its removal is usually desirable, for the recovery of coins and datable pottery from beneath it would be a valuable, and at present all too rare, addition to our knowledge of the history of the mosaicist's art. To preserve it *in situ* robs us of such direct knowledge, and involves considerable expenditure in building a hut over the floor and in making the site weatherproof. Moreover damp may still rise through the floor and in time cause damage: frost can play havoc with a damp floor, and *algae* will appear to discolour it. Reburial of the floor or its destruction are mere counsels of despair.

The classic description of a modern method of raising a mosaic is that by Mrs T. V. Wheeler in *The Museums Journal*, XXX (1933), pp. 104 ff., and it has been reprinted by Sir Mortimer Wheeler in *Archaeology from the Earth*, pp. 111–13. It is a method of some elaboration, but its success can be judged by a glance at the splendid mosaics now preserved in the Verulamium Museum.

Apart from its elaboration, however, the method has one or two disadvantages. In the first place it involves the use of glue, which is difficult to melt and apply in the open air. A more serious disadvantage is the need to dig a trench 1 ft. wide by 18 in. deep round the floor to drain it, together with the disturbance caused by pushing planks beneath the floor in order to reverse it. Both these operations are liable to disturb the lower layers which may contain the vital coins or pottery. Finally, large floors have to be dismembered, and the cuts can never be quite eradicated in restoration.

In 1957 a mosaic floor was found in Building 3 of Insula XXVII at Verulamium,[1] and since it appeared desirable to preserve it in the Museum, a new method was evolved of dealing with it by means of a plastic solution using a volatile solvent. I was fortunate in having the help on the site of Mr W. S. C. Kennett of the Plastics Division of Imperial Chemical Industries Ltd., and to him is due the credit for evolving the method and of providing notes on which the first part of this article is based. The work was superintended by Dr Norman Davey, F.S.A., who was responsible for the remounting, and he has kindly sent me details of the method used. As a preliminary we experimented with rubber latex on a small outlying piece of plain floor, but found it did not grip.

The mosaic was first cleaned and washed thoroughly for photography, and was then allowed to dry. We were fortunate in the weather, so that the only precaution taken was to spread a tarpaulin over it at night. Complete dryness can be tested by applying some of the plastic solution to a small piece of floor: if the floor is still damp, the film will turn cloudy. In one or two areas we applied methylated spirit and ignited it to hasten drying.

As soon as the floor was dry, it was given a good brushing with a stiff brush, making sure that as much earth as possible was removed from the interstices. This step is important

[1] Interim report forthcoming in *Antiq. Journ.*, XXXVIII, January/April 1958. The floor, a late one, was only just below the plough-soil, and had been cut into by the foundation trenches of a medieval timber cottage.

as the absence of earth enables the adhesive to penetrate round each tessera: thus a good bond is obtained.

The adhesive used had the following formulation:

P.V.C. co-polymer ' Corvic ' Q44/62	100 parts	by weight
Methyl ethyl ketone	500 parts	
Toluene	100 parts	
Dioctyl phthalate or dialkyl phthalate	10 parts	

' Corvic ' Q44/62 is the registered trade name for a co-polymer manufactured by Imperial Chemicals and may be obtained by writing to the following address: Imperial Chemical Industries Ltd., Plastics Division, Black Fan Road, Welwyn Garden City, Herts. (for attention of Technical Service and Development Department).

The solvents and plasticizers are obtainable from British Industrial Solvents Ltd., Hull.

The correct amount of co-polymer is shaken, or preferably stirred by a mechanical stirrer, with the calculated amount of methyl ethyl ketone and toluene. This may take some time if merely shaken. The plasticizer (dioctyl phthalate) is then added and stirred into the solution.

Once the floor was ready, a thin coat of the solution was spread on its surface with a trowel, making sure that all cracks were filled and all surfaces wetted. This coat was allowed to dry before the work was continued. A second coat was then applied very thoroughly, and this was left until a hard skin had been formed so that it was possible to walk on it without breaking this skin. At this point a thin cotton cloth of close weave was laid over the treated floor. This material was obtained from a draper's store and was 82 in. wide; but if the cloth does not cover the floor an overlap of about 2 in. is required. The solution was poured over it starting at one end, the cloth being pulled so as to even out the creases, so that it could set under tension. Care was taken to ensure that the cloth touched all parts of the floor.

In all, two gallons of solution were used on the floor, the area under treatment being 5 ft. by 9 ft. The whole was left to dry out for 18 hours.

Once the adhesive was dry, the raising of the floor was begun. This consisted of undercutting the mosaic and loosening the mortar basis with a trowel: the freed mosaic was then pulled back to expose more uncut mortar. In practice this mortar basis was very decayed owing to local conditions and was easily loosened with two or three strokes of the trowel; with more intractable material a cold chisel and hammer might have to be employed. As the process continued, the floor was rolled back over two cardboard drums and then passed, cloth down, on to a sheet of hardboard for transportation to the Museum (PLATE XV, *a*). As a matter of fact, it was only just possible to manoeuvre it through the doors of the Museum, and we concluded that it would have been better, and in the case of a larger area of floor necessary, to roll it up like a carpet. The tensile strength of the p.v.c. sheet was sufficient for such treatment.

Once in the Museum, the back of the mosaic was thoroughly cleaned, especially the interstices, with brushes, knife blades, and even a vacuum cleaner. Meanwhile a shallow wooden frame was constructed with a plywood base, in which the mosaic was laid face downwards. The frame, 2 in. deep, was mounted on bearers which were themselves supported on trestles (FIG. 1, I). Next a fillet of Portland cement mortar (1 part cement to 1 part sand by weight) was put round the edges of areas where tesserae were missing, and these areas were then filled with dry shingle (FIG. 1, II). This was to prevent the backing next to be applied from penetrating to the front through these gaps. First, however, a slurry of Portland cement and water was brushed well into the back of the mosaic, after

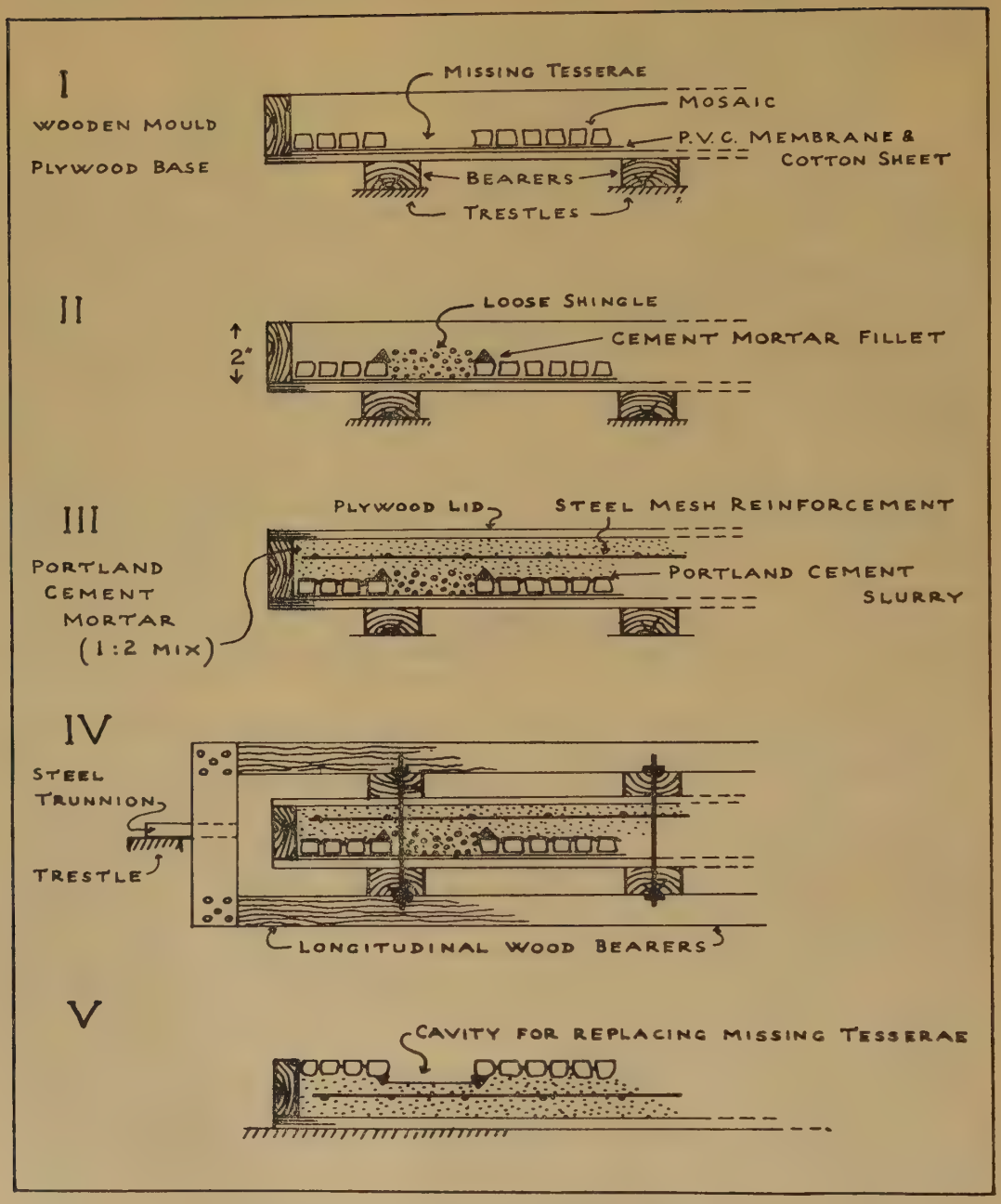


FIG. 1. Sketch to illustrate stages in the process.

which the backing, of Portland cement mortar (1 : 2 mix) was spread to a depth of 1 in. On this was laid a steel mesh reinforcement (B.R.C. 6 in. square mesh) and then more cement was applied, tamped well into position and screeded off level with the top of the mould.

Once this bed of cement was dry, a lid of plywood was affixed (FIG. 1, III). To reverse the floor additional cross bearers were added above and bolted to those already in position below. A longitudinal bearer to act as axle was then fixed centrally to the cross bearers on each side by steel brackets: these longitudinal bearers had special end-blocks carrying a steel trunion. Under these trunions trestles were placed at each end and the whole slab reversed by rotation (FIG. 1, IV, and PLATE XV, *b*). The longitudinal bearers were removed and the slabs (which weighed about $\frac{3}{4}$ ton) lowered to floor level by jacking down.

Next the plywood lid was removed and the p.v.c. sheet stripped off in ribbons without the use of solvent. At first solvent was tried, but it was found that a much cleaner job resulted from just ripping the sheet off in strips: it brought with it some of the residual dirt. When the loose shingle was brushed away cavities were left in the gaps in the mosaic which could be made good either with spare tesserae set in cement slurry, or with a plain surface of cement, which in turn could be later painted or left plain (FIG. 1, V).

The floor now lies in the Verulamium Museum. It is still all in one piece as its makers made it, and with the slight irregularities of its surface it lacks the smoothly artificial aspect of the normal rebuilt pavement. To its excavator it looks, in fact, like what he remembers digging up.[2]

SHEPPARD FRERE

[2] A 16 mm. film in colour was made for the Institute of Archaeology by Mr M. B. Cookson illustrating all phases of the operations described here.

BYZANTINE EXHIBITION, 1958

An exhibition of Byzantine art is being arranged for this summer; it will open in Edinburgh on 22 August, as the principal ' Art ' activity of the International Festival. After three weeks it will be transferred to the Victoria and Albert Museum in London; it will open there on 30 September and will close on 8 November.

The primary object is to give a picture of the Art of Constantinople between the foundation of the city as the capital of the Christian world in 330 and its conquest by the Turks in 1453. A few objects of early date from other places will however be included in order to bring out the essential characteristics of the Constantinopolitan style, while a number of paintings and icons of late date will also be shown, to illustrate the legacy of Byzantium after the Turkish invasions.

Objects are being loaned from museums and Cathedral Treasuries all over Europe, and will include some of the finest works that have come down to us. Especially noteworthy are pieces of silver plate from Nicosia in Cyprus, from the Constantinople Museum, and from the Hermitage Museum in Leningrad, which are not likely to have been seen by more than a very few people in western Europe owing to the inaccessibility of these places so far as the average traveller is concerned. Some fine ivories and a number of painted panels—icons—dating from the 12th and following centuries, are also coming from the U.S.S.R.; they are of outstanding interest for the same reason and also because it is there that the finest examples of these early panels are preserved. Some pieces of silver, jewellery and other notable objects which are not likely to be familiar to more than a few are also coming from Turkey.

Loans from western Europe include ivories of early date, notably Consular diptychs, as well as ones of the 10th, 11th and 12th centuries, the so-called ' Second Golden Age ' of

Byzantine art. Notable church treasures (from St Mark's at Venice, Nonantola, Capua, and elsewhere in Italy), textiles (from Belgium, Holland and France) and ivories (from Berlin, Florence, Milan and numerous museums in Germany, Italy and elsewhere) will form the bulk of the exhibition; but many of the finer Byzantine objects from the British Museum, the Victoria and Albert, and other museums and private collections in Britain are also being made available. There will also be a representative collection of Byzantine coins.

This is the first exhibition of its kind to be held on British soil and it is to be numbered amongst the very few similar enterprises that have taken place in Europe. The only other Byzantine exhibitions have indeed been that, on a fairly small scale, held at Grottaferrata in Italy in 1905, and that, on a larger scale, but including also a large number of related material from Coptic Egypt and elsewhere, which was held in Paris in 1931.

INTERNATIONAL CONGRESS OF CLASSICAL ARCHAEOLOGY

The Eighth International Congress of Classical Archaeology will open in Rome on 6 September, 1958, and will then continue in Naples from 9 to 13 September. The Secretary of the Congress, from whom all details may be obtained is Professor Pietro Romanelli, 49, Piazza San Marco, Rome. There are four post-Congress tours beginning on 14 September. Five principal subjects will be discussed at the Congress: (1) new methods in archaeological research including aerial photography and under-water archaeology; (2) new discoveries in the archaeology of Greece and the Hellenic East; (3) the archaeology of pre-Roman Italy; (4) new problems in Roman archaeology; and (5) the spread of classical civilization outside the classical world to Iberia, Britain, Central Europe, the Black Sea and the Middle and Far East.

COWA

The Council for Old World Archaeology was formed to advance the study in America of the archaeology of Europe, Africa, Asia and Oceania. It is a non-profit making corporation, control of which is vested in representatives from nine American learned societies. Itself it is not a learned society but an ' agency for service ' and its aim is to publish biennially for every part of the old world a survey of current archaeological activities and a current selected and annotated bibliography. The Council has especially in mind four classes of readers: archaeologists specializing in one area or field who want to know about other areas, scholars in other related disciplines who want to know about archaeology and ancient history, ethnologists and sociologists, and intelligent laymen. Lauriston Ward is President and Editor in Chief of COWA. The expenses of the Council during its organization period have been met by gifts from individuals and by a special grant from the Wenner-Gren Foundation for Anthropological Research. The Old World has been divided into twenty-two areas, and the COWA survey and the COWA Bibliography will be issued in groups by areas, five or six areas being covered every six months. At its meeting in Madrid in 1954 the International Congress of Prehistoric and Protohistoric Sciences passed a resolution endorsing the aims of the Council for Old World Archaeology and invited the members of the Congress to co-operate with it. The first group of COWA publications is now in print and being distributed. It consists of the Survey and the Bibliography for the areas of Central Europe, Eastern Mediterranean, North-West Africa, Northern Asia, Western Asia and Indonesia. The second group (Survey and Bibliography for British Isles, European Russia, West Africa, Southern Asia and Pacific Islands) will be issued in April or May, 1958. The Council emphasizes that its available funds are not sufficient to permit any free subscriptions anywhere, or any subscriptions on the basis of the exchange of

publications. Subscribers should write to the Council for Old World Archaeology, 11, Divinity Avenue, Cambridge 38, Mass, U.S.A. The annual subscription (covering eleven areas issued in two groups) is four American dollars. A preliminary look at what the Council has already published shows that, although intended for America, its surveys and bibliographies will be indispensable for all archaeologists working in the Old World. We have no comparable survey and bibliographical machinery; COWA is just the learned service agency for which so many of us have been hoping for so long. The list of its Trustees inspire confidence, and its first sample publications demonstrate that this confidence is not misplaced.

THE CARNEGIE TRUST AND ARCHAEOLOGY

Since 1951 the Carnegie United Kingdom Trust has had a policy of grant-aid for groups of amateurs practising the visual arts. During 1957 the trust embarked on a policy of grant-aid for amateurs whose spare time interests lie in science rather than the arts, and, avoiding for the time being the physical sciences, has decided to begin with activities in natural history and archaeology which take amateurs into the countryside. The policy is in three parts, all with the object of assisting the group activities of societies whose members are mainly, if not entirely, amateurs. The first part of the policy is the offer of bursaries to adult amateurs to enable them to attend courses offered at Field Studies Centres. The third part of the policy is an attempt to improve the record of information about local scientific societies by preparing, through the British Association for the Advancement of Science, a directory of societies and unions of societies. It is the second part of the policy that will most interest readers of ANTIQUITY; it is a scheme devised in conjunction with the Council for British Archaeology of grant-aid for amateur field work in archaeology. To quote from the Trust's 1957 report ' the object of this scheme is to encourage local archaeological societies to arrange practical group activities among their members not limiting their outdoor work to excavation only, but taking in general field work as well. Grants will be offered primarily for the fees and expenses of the directing staff of approved projects, and it is hoped that this offer may encourage enterprising groups to tackle work they have not so far been able to contemplate. Grants will also be available towards the purchase of equipment for particular projects. The emphasis on this scheme is on the practical side of archaeology and passive activities will not qualify.' The Trust emphasizes that it is not its intention to compete with the training schools organized by University Extension Extra-mural Activities Departments. Applications for grants (which should be from recognized societies or groups of societies and which should involve projects mainly for amateurs) should be made in the first instance to the Council for British Archaeology, 10 Bolton Gardens, London, S.W.5.

AN EARLY BRONZE AGE KRAAL AT BISKUPIN PLATE XVI

In the years 1952–54 excavations were carried out on one of the hills near the well-known Lusatian earthwork stronghold of Biskupin in North-West Poland (ANTIQUITY, 1938, 311). Long ditches with deep deposits containing Early Bronze Age finds were discovered, and in 1956 six weeks' excavations were undertaken by the State Archaeological Museum in Warsaw. The ditches are situated on the highest sandy hill on the shore of the Biskupin lake. It is surrounded on three sides by low peaty meadowland. On the fourth side the hills are connected with an upland of rich soil (PLATE XVI, *c*). The original, upper layer of the sandy hill, 1·5 m. deep, consisted of alternate thin layers, 20–50 cm. thick, of sand and clay. Here and there occur traces of moraines, stratified with sand, clay, gravel and boulder. The hill, which eighty years ago was birch-forest, is now under cultivation.

The complicated geological system of the upper strata made for many difficulties in the field. In 1956 an attempt was made to discover the function of the ditches. Trenches were cut across them at intervals varying from 5 to 20 m. Twenty such trenches were cut during the six weeks. Parts of the ditches on the eastern and southern sides were completely excavated (PLATE XVI, *a*, *b*). Difficulties were caused by the ditches being hardly noticeable on the surface, sometimes indistinguishable even at the depth of 0·8–0·9 m. The upper portions of the ditches had been washed away by the action of surface water from the steep top of the hill. Only the lowest portions of the ditches, on an average 0·3–0·5 m. above the bottom were well preserved.

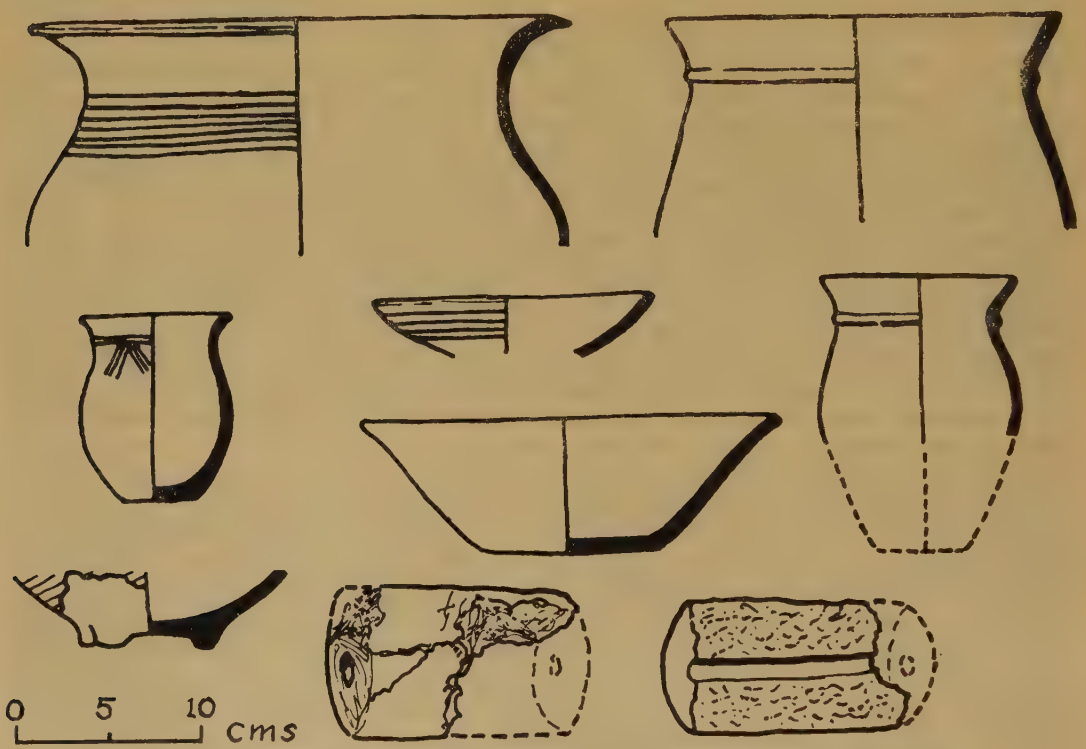


FIG. 1. Pottery and loom-weights of the Iwno Culture from the kraal.

The total length of the ditches was about 250 m. The chief ditch embracing the whole area was 210 m. long; the shallower outer one, situated on the southern, more accessible side, 40 m. long. The supposed original depth of the steep-sided main ditch was about 1·7–1·9 m., that of the shallower outer one about 1·5 m. Hearths irregularly placed just above the bottom were discovered in both ditches. To date six hearths have been found in the chief ditch. Four of them were placed near narrow 1 m. wide causeways running across the ditch from the side of the old lake. Two other hearths in the north-eastern part aroused special interest because two pairs of damaged, oblong, 10–12 cm. long loom-weights were found in them (FIG. 1). These may be related to primitive vertical looms producing textiles not more than 24 cm. wide. Bones of domesticated animals were also found in the hearths: cattle bones mainly, with swine and sheep next. There were, moreover, great quantities of pond-mussels and of bones of common fishes such as perch,

pike and bream, around the hearths. Many pottery fragments occurred in the hearths and at the bottom of the ditches. Pots with cordons round their necks, and dishes, were the most common forms. Thin-walled vessels ornamented with grooves and tassel-like motifs, characteristic of cemeteries of the period, were rare. Several small irregularly shaped implements (most of which were scrapers), two cores, several flint flakes, and one antler chisel were found. A plain bronze pin with a thickened upper part was discovered in one of the hearths.

In the plan (PLATE XVI, *c*) the area embraced by the ditches (about 3,900 sq. m.) was roughly oval, about 90 m. long and 35–50 m. wide. The flat top covered about 900 sq. m.; the western slope 1,200 sq. m., and the eastern slope 1,900 sq. m. The greatest difference between the levels within the area was 2·5 m. The main ditch had two 15 m. wide causeways (gates). The shallow, outer ditch consisted of three parts, several metres long. Two of them run parallel to the southern part of the chief ditch for a distance of 1·5 –3 m. The third part was situated opposite to the chief southern entrance (gate) for a distance of several metres.

The ditches must have belonged to a kraal for animals, chiefly cattle. It is situated near the lake and the low-lying meadow land. Owing to the continuous winds there were probably no gad-flies on the hill. For this reason a great quantity of cattle could be kept in the kraal, in spite of the near vicinity of swamps. The steepness of the kraal probably did not create difficulties, as some of the profiles suggest that a low earth rampart was made from the soil dug out from the ditch. The rampart would perhaps have been strengthened with a hedge or fence of blackthorn. Cattle and other animals herded here together all night long must have completely denuded the kraal of vegetation. In consequence the upper layers of the hill and a part of the ditches were washed away. Shepherds seem to have lived in sheds built probably directly above the ditch over the hearths. The causeways were probably closed with a fence of wattle-work. About 500–800 head of cattle could be kept in the kraal. The works having been repaired several times, it is possible that the breeders returned to their base settlement each autumn. Their food consisted of the meat of domestic animals, with pond-mussels, fish, and probably also corn.

The Early Bronze Age kraal of Biskupin is the first of its kind in Europe to be examined almost completely. The finds enable it to be dated to the end of the first period of the Bronze Age (according to Montelius). They belong to the local West Polish-Kuyavian culture of Iwno which was contemporary with the Unětician culture.

ALEXANDER GARDAWSKI

ARCHAEOLOGY IN OXFORD UNIVERSITY: TWO RECENT DEVELOPMENTS

1. *The Research Laboratory for Archaeology and the History of Art*

The project of a Research Laboratory to work on the archaeological applications of physical science was first put forward in Oxford in 1950, and was finally realized in the early part of 1955, when, after receiving grants from the Wenner-Gren Foundation and the Nuffield Foundation, the University created the Laboratory by statute and installed it in its premises, 6 Keble Road. A chief part in its creation was taken by the late Viscount Cherwell, who was then still the professorial head of the Clarendon Laboratory, and had long seen what archaeology and art-history would stand to gain from analytical treatment of their materials by the techniques of physics. The new laboratory's governing committee includes the present holder of Lord Cherwell's post (Prof. B. Bleaney) and the Professor of Geology and Mineralogy (L. R. Wager), while its chairman is the Keeper of the Depart-

ment of Antiquities in the Ashmolean Museum (at first D. B. Harden, and from the end of 1956 R. W. Hamilton), and its other members are the Keeper of the Department of Fine Art in the Ashmolean (K. T. Parker), the Curator of the Pitt-Rivers Museum (T. K. Penniman), and the Professor of European Archaeology (its secretary, and writer of these lines); Mr Peter Swann of the Museum of Eastern Art is also a member, and the work of the Laboratory has the co-operation equally of the scientific and of the archaeological and museum sides of the University. It is indebted likewise to a number of university and other institutions and museums outside Oxford, and for benefactions to the generosity of Major H. W. Hall and of several grant-giving funds and foundations, as well as of the two above-mentioned and of the University itself. The staff consists now of two Senior Scientific Officers, two Research Assistants, three technicians, a secretary and two part-time workers (optical spectrometrist and statistician). The first of the two officers, Dr E. T. Hall, is the executive director; the second is Dr M. J. Aitken, who succeeded in 1957 to Dr S. A. Young.

The work that is being carried out includes, in the first place, X-ray Spectrometry, which, being non-destructive, is particularly suited to the analysis of specimens of precious metals, notably coins, and of glass and glazes, as was demonstrated in the Laboratory's first published paper, on the chronological determination of Chinese Blue-and-White ceramic with reference to the manganese content of its cobalt blue. (See Stuart Young in *Oriental Art*, II, 2, Summer 1956, pp. 43–7, ' An Analysis of Chinese Blue and White ', together with Sir Harry Garner, pp. 48–50, ' The Use of Imported and Native Cobalt in Chinese Blue and White '.) Two X-ray Fluorescent Spectrometers, scanning under automatic control, have been built successively to Dr Hall's designs. In Optical Spectrometry, a programme of analysis of prehistoric European bronzes of the Middle and Late Bronze Ages is being carried out, with the co-operation of the Ashmolean, Pitt-Rivers, and British Museums, the Museum of Archaeology and Ethnology at Cambridge and several other museums; and also one on early Chinese bronzes. Magnetic dating of ceramic and other clay products is being conducted on an Induction Magnetometer, of a highly sensitive design elaborated with the co-operation of the Institut de Physique du Globe, Paris, and the Cambridge University Department of Geodesy and Geophysics; Dr Aitken has been especially engaged on the application of this method of dating (by measurement of the thermoremanent magnetism in the clay) to Chinese Yüeh ware. Neutron Activation Analysis, a new technique, equally non-destructive, of analysing archaeological specimens by measurement of concentrations of trace-elements, is proceeding with use of one of the Harwell nuclear reactors for the neutron irradiation of the samples; the decay-curves of the gamma-ray activity so induced are identical for samples of identical composition, and the characteristics of the trace-elements present are detected by a special Gamma-ray Spectrometer. The method is applicable, for example, to pottery and also to coins, on which work is now in progress. A further non-destructive technique is that of Optical Spectrophotometry, for which an instrument designed by Dr Hall has been built to analyse the inks and pigments of written documents and paintings by evaluation of their reflected light. Lastly, in the field, work has been done not only in earth-resistivity surveying of sub-surface features, but also in their magnetic detection, using a Proton Resonance Magnetometer, which appears well suited to ascertaining the outline of such buried structures especially as kilns.

The purpose of these notes is simply to make the Laboratory's existence more fully known among the readers of ANTIQUITY, and to illustrate the range and variety of the field—the application of physics to archaeological and art-historical problems—in which it has accomplished still only its first three years of work. But arrangements are now being

made for making known specific results attained from time to time. Dr Hall and his team are getting well into their stride, and the news of their progress at intervals should rouse interest in many quarters.

2. *The Diploma in European Archaeology*

On 11 March this year the University enacted a statute establishing a Diploma Course in European Archaeology, side by side with the existing one in Classical Archaeology, and replacing the existing Committee for Classical Archaeology by a new ' Committee for Archaeology ', to cover the whole European as well as the Classical field and to administer both the courses. Its chairman (strictly, as deputy for the Vice-Chancellor) will be the Lincoln Professor of Classical Archaeology and Art (B. Ashmole), and its secretary the Reader in the same subject (W. Llewellyn Brown). Its members are the professors of European Archaeology, of Ancient History, and of Celtic, representatives of the Faculties concerned (Literae Humaniores, Modern History, English, and Anthropology and Geography), some co-opted members, and the Keeper of the Department of Antiquities in the Ashmolean Museum (R. W. Hamilton).

The general field of study for the new Diploma is Europe from the Neolithic onwards, outside the higher Mediterranean civilizations, but with concern for all connections with them; further, a candidate must specially study some one European area, and one of four periods, running in succession (with slight overlaps) from the Neolithic and Early Bronze Age to the Early and the later Iron Age (to A.D. 700). The special period to be prescribed initially is the second of these, running from the 13th to the 5th century B.C. Some capacity for reading Continental literature in the original is expected; in particular, a reading knowledge of archaeological German, which is really not very difficult to acquire. Candidates have to be, or to become, members of the University; the statute comes into effect on 1 October, and the first examination (if there are entries) will be in June 1959. The Professor of European Archaeology and his Assistant (Miss M. A. Smith) now have official quarters at 35 Beaumont Street, together with the Professors of the Archaeology of the Roman Empire (I. A. Richmond), of Chinese (Homer Dubs), and of the History of Art (Edgar Wind). This is very close to the Ashmolean Museum, and the collaboration of the Museum staff is promised in making the collections to a greater extent available for teaching. (Of prehistoric European antiquities, the Senior Assistant Keeper in charge is H. J. Case.)

Prehistory in the ' primary ' sense, beginning with the Palaeolithic, not geographically confined, and maintaining its traditional conjunction with Ethnology and Social and Physical Anthropology, continues meanwhile to have its Oxford home in what is now styled the Department of Ethnology and Prehistory, with its quarters and collections in the Pitt-Rivers Museum. Under the Curator (T. K. Penniman), archaeological matters there are mainly the concern of J. S. P. Bradford, while an Honorary Lecturership in Prehistoric Archaeology is held by D. F. W. Baden-Powell, covering the Palaeolithic and Mesolithic periods and Quaternary studies in general. The graduate course in Prehistory in this sense is taken as one of the three options for the Diploma in Anthropology; for Europe, in and after the Neolithic, its wider field meets that of the new Diploma course. Lastly, while the origins of civilization in the Near East lie within it too, the archaeology of civilization as developed there, and in the East at large, belongs to the Faculty of Oriental Studies.

Side by side with this and with Classical Archaeology, and forward from primary Prehistory, the new course sets the archaeology that is specifically European, continuing what Gordon Childe named the Dawn of European Civilization from the 2nd to the 1st

millennium B.C., and onwards, alongside Greece and Rome, to the brightening dark of its medieval morning. What remains to be seen is whether anyone will take it. But we shall be ready for those that will; and indeed we have not lacked time for preparation. The need for something of this kind in Oxford began first to be pointed out (as Dr Joan Evans has reminded me) by Arthur Evans in 1888—just seventy years ago. Yet the tortoise, in the end, did win the race; and we shall try.

C. F. C. HAWKES

THE SOCIETY FOR MEDIEVAL ARCHAEOLOGY

In the summer of 1957, at a meeting held at Burlington House under the chairmanship of Sir Mortimer Wheeler, a new national archaeological society was founded. This, the Society for Medieval Archaeology, exists to further the study of Britain's post-Roman history by encouraging research into the surviving material evidence. The Society's activities will encompass the archaeology of the Anglo-Saxon period in Britain, in all its aspects. The period after the Norman conquest will be treated within more strictly archaeological terms of reference, so that attention will be focussed on fieldwork and excavated material rather than on those studies of art and architecture to which other societies and journals are already devoted.

The Society was founded in the belief that a centralized medium of publication would stimulate and unify contributions from the various disciplines involved. Thus, its chief purpose is to publish a journal in which the problems and results of archaeological, historical, numismatic and linguistic studies of the medieval period in Britain will for the first time be stated and discussed together. The archaeology of the European mainland will be treated in so far as it bears on the British problems and material. In short, this publication, which is to be called the *Journal of Medieval Archaeology*, is intended to be truly national in scope and international in significance.

The Society will arrange an annual week-end conference. The first of the series was held in March 1958 at Sheffield, its subject being the relationship of history and archaeology in the study of the Middle Ages.

There will be an annual general meeting in London. In December 1957 a paper entitled *Some Comments on the Miniatures and Ornaments of the Lindisfarne Gospels* was read to the first of these meetings, by the President, Mr R. L. S. Bruce-Mitford, Keeper of British and Medieval Antiquities in the British Museum.

The other officers of the Society are : Secretary, Mr D. M. Wilson, of the Department of British and Medieval Antiquities in the British Museum; Editor, Dr D. B. Harden, Director of the London Museum; Treasurer, Mr J. G. Hurst of the Inspectorate of Ancient Monuments of the Ministry of Works. Sir Cyril Fox, Sir Thomas Kendrick, Sir Frank Stenton and Sir Mortimer Wheeler are Honorary Vice-Presidents.

Membership is open to all who are interested. The subscription to the Society, including the *Journal*, for ordinary and institutional members is £2 2*s*. 0*d*.; for *bona fide* students under the age of twenty-five £1 1*s*. 0*d*.; and for two members of the same household £3 3*s*. 0*d*. (entitling them to full privileges of membership and one copy of the *Journal* jointly). Further particulars can be obtained from the Secretary, at the British Museum, W.C.1.

ARCHAEOLOGY IN WALES

There is good news for those who practise archaeology in Wales or who want to know about the ancient history of western Britain. At long last, the University of Wales has established a Professorship of Archaeology, and we welcome Mr R. J. C. Atkinson, at

present Lecturer in Archaeology at Edinburgh, as Professor of Archaeology in the University College of South Wales and Monmouthshire at Cardiff. It may come as a surprise to many readers of ANTIQUITY outside Great Britain that a University of Ancient Britons has waited sixty-five years before it has recognized officially the study of its own remote past. The ghosts of Edward Lhwyd and Sir John Rhŷs will haunt with pleasure the hall at University College, Cardiff, when Professor Atkinson gives his inaugural lecture.

THE SCHAFFHAUSEN CELTIC EXHIBITION

The Keeper of Antiquities in the Ashmolean Museum kindly points out that our note on the Schaffhausen Exhibition in the last number of ANTIQUITY was misleading in one detail. Three objects from Oxford were exhibited (although they did not appear in the printed catalogue), namely the Minster Ditch scabbard and two bronze ' spoons ' from Penbryn. We are glad to be able to add the Ashmolean to the very small list of those museums and private individuals who sent material from Britain to this Celtic Exhibition.

THE ANGLO-SAXONS AND THE B.B.C.

Two years ago the B.B.C. arranged a series of special broadcasts on Roman Britain, and last year a similar series on the Anglo-Saxons. This year it is to be the Normans. These broadcasts are seldom discussed outside the broadcasting review columns of the daily and weekly papers; yet serious discussion is wanted and welcomed by producers of these programmes and of The Archaeologist *on Network Three and* Buried Treasure *on B.B.C. Television. We are happy to print here the comments of Miss Rosalind Hill of Westfield College on the Anglo-Saxon broadcasts.*

The work of the B.B.C. in stimulating a popular interest in archaeology has been an unqualified success. Indeed, we have reached the stage when archaeologists have to be cautious in mentioning new discoveries for fear lest the inrush of enthusiastic spectators may hinder their work.

Can the same interest be aroused for the study of history, and in particular for the history of England between A.D. 597 and 1066? In a recent series of broadcast talks the B.B.C. obtained the services of a body of learned men, each of them a first-rate authority in his own field, who held eight discussions on the subject of Anglo-Saxon England. The result, according to evidence obtained by the producers, was to give great pleasure to people with some knowledge of the subject, but not to capture the imagination of the general public. My own limited researches among university students tend to confirm this opinion. Those who knew something of the outlines of Anglo-Saxon history were interested, even to the extent of regretfully foregoing their suppers. Those who knew nothing of the period were completely baffled.

It was unfortunate that the course was started in the middle of the period with the reign of Alfred, ' the one person in whom the [English] sense of belonging can be personified '. Most English people under the age of forty have been brought up without any strict training in historical chronology, with the result that their ideas of history are inevitably muddled. Nevertheless, most people understand a story better if it has a beginning, a middle and an end. The use of the ' flash-back ' seems to be of doubtful value even in romance, and it can be extremely unsafe in history unless the student has already grasped the main outlines of the period. An intelligent person with no specialized knowledge could not but be interested to hear an expert lecture on Bede's Northumbria or Alfred's Wessex; he could hardly be expected to assimilate a few scholarly remarks on both, presented in reverse order with no very clear indication of the relationship between them.

The whole popularity of the ' Brains Trust ' or ' Any Questions ' programmes seems

to arise from the fact that each question can be answered separately without reference to the next, and therefore such programmes are extremely suitable for discussions between people of wit and wisdom. History does not work out in this way, and it cannot be tabulated under a series of convenient labels. It seems likely that the subject can best be presented to a popular audience if the experts are prepared to take a strictly limited period (probably of not much more than a century) and to start by explaining, *viva voce* so as to catch the attention of the experimental listener, the obvious background of written sources and accepted dates upon which they propose to work. The quick and allusive style of the Anglo-Saxon broadcasts delighted some historians and infuriated others, but it could not reasonably be expected to capture the interest of the average listener who knew nothing of the subject. A more satisfying result might be obtained if each expert were invited to lecture, in a simple style with as few technical phrases as possible, upon his own subject, and not forced to pour a gallon into a pint pot. Bede's *Ecclesiastical History*, for example, is probably the greatest historical work produced by an Englishman, and as a literary achievement it is at least as outstanding as Chaucer's *Canterbury Tales*—but who would claim the power to introduce a reader to Chaucer in the course of half a discussion lasting, in all, twenty-eight minutes ? The account of the excavations at Yeavering, set in their historical perspective, kept a highly-trained audience at a meeting of the Society of Antiquaries on tiptoe for more than an hour. Could the same material really be made intelligible to the man who was not a specialist in the course of a quickly-delivered conversation of about twelve hundred words ?

The capacity of the ordinary listener should by no means be underrated. Whether in breeding budgerigars or in studying parish registers, the amateur often shows a capacity for patience and thoroughness which should put many professionals to shame. But the amateur historian will rarely have studied the history of pre-Conquest England, nor is he likely to be spurred into action by generalizations, however sound, or by great numbers of isolated facts, however interesting. He must be able to relate them to some clear and solid background of time and space. If he studies the B.B.C.'s pamphlet on *Anglo-Saxon England* he will find much, including an excellent bibliography and a time-chart, to help him, although he may be puzzled to see that the adjective ' Celtic ' is defined as, *inter alia*, ' the artistic style typified by Anglo-Saxon crosses and illuminated MSS ', and that two separate Runic symbols are said to denote the sound of ' th ' as in ' three ' and ' thin '—not, as is surely true, the sound as in ' thin ' and ' those '. The trouble is, however, that he will not send for the pamphlet, or read the books, unless his imagination has first been caught by the broadcasts.

Might not much greater success be obtained by taking one limited field—for example the history of Northumbria between the arrival of Paulinus and the death of Bede—and asking each scholar to lecture on his own particular subject within it ? This might lead on to an extremely valuable symposium planned with some idea of the questions which listeners wanted to ask. The present method of discussion, excellent for archaeological subjects, does not seem to be suitable for the teaching of history, even in the hands of historians who are masters of their craft.

ROSALIND HILL

Reviews

CATALOGUE OF MANUSCRIPTS CONTAINING ANGLO-SAXON. *By* N. R. Ker. *Oxford, Clarendon Press*, 1957. *pp.* lxiv + 568 *and* 8 *plates.* £5 5*s.*

It would be difficult to exaggerate the importance of this work, which will be an essential book of reference for generations of Anglo-Saxon scholars, whether palaeographers, linguists, historians or students of literature. It provides a corpus of all Anglo-Saxon entries in manuscripts, including even occasional glosses and scribbles, with the exception of cartularies and of charters which survive on separate parchments. It contains a most valuable palaeographical introduction (to which the eight plates serve as illustration), in which, among other things, Mr Ker's unrivalled familiarity with the whole body of the material allows him to distinguish the styles of some scriptoria, such as Winchester and Exeter. Many hitherto unlocated manuscripts can now be allotted with certainty or probability to a place of origin. Another valuable feature is the reassembling of dismembered manuscripts, a service which Mr Ker has performed in the past in various articles and notes. It is convenient to have these, and similar findings of other scholars, brought together in one book, together with new discoveries of this nature. Among these, one of special interest is the suggestion that the ' F ' version of the Anglo-Saxon Chronicle (Cotton Domitian viii) formed part of the Cambridge University Library MS. Hh. i. 10, and thus is the ' Historia Angliæ Sax.' mentioned in the list of Parker's gifts, and hitherto believed to have been lost. No less valuable is information that manuscripts which have been regarded as a unity are made up of separate works, bound together later, often in early modern times. After the catalogue proper, Mr Ker adds a remarkably wide and comprehensive bibliography, which will draw attention to many works whose significance has been either overlooked or forgotten, and this is followed by a series of useful indices to help the scholar in any field to find what he wants among the truly amazing mass of information contained in the body of the work. It would be impossible in a review to do more than select for illustration a few of the many contributions this work makes to our knowledge.

Mr Ker begins with an appreciation of his great predecessor, Humphrey Wanley, and claims that the latter's work is not superseded by this new catalogue. But much has come to light since Wanley's time. Manuscripts not described by Wanley include such important ones as the Epinal Glossary (114),[1] the Vercelli Codex (394), the Leningrad Bede (122), the *Liber Vitae* of Hyde Abbey (274), the Sherborne Pontifical (364), the Bosworth Psalter (129), the Tanner and Corpus Christi, Oxford, MSS. of the Old English Bede (351, 354), the oldest version of the bilingual *Rule of St Benedict* (353), the oldest manuscript of Aelfric's Grammar (362), and the Stowe Psalter gloss (271). Several of these have, of course, been well described elsewhere, and it is perhaps in his collection and description of minor and fragmentary items that Mr Ker most advances scholarship. Thus he draws attention to a fragment of the West Saxon Gospels in private possession (112), to a leaf with twenty-five lines of Ovid's *Ars amatoria*, with Old English scribbles on the verso (297), to a version of Caedmon's Hymn not included in Dobbie's list (387), to a mid-11th-century fragment of a *Legend of the Cross* (73), a work otherwise known only in a 12th-century

[1] Numerical references are to those in Ker's catalogue.

copy, and to a leaf affording another example of scribbles in a hand which Mr Ker has shown to be either Archbishop Wulfstan's or that of someone very closely associated with him (130). Various fragments of the works of Aelfric are included, i.e. in entries 74, 81, 235, 242, 243, 285, of which only those in 235 are in print, while the last, a part of the homily for Septuagesima Sunday in the second series of *Catholic Homilies*, now in private possession, has variant readings from the versions previously known. The above list, a selection only, leaves out those fragments such as *Waldere* (101), *Finnsburh* (282), the *Battle of Maldon* (172), the Kassel leaf of the *Regula pastoralis* (195), the early 10th-century fragment of Alfred's *Boethius* (337), parts of the Old English *Martyrology* (127, 132), version ' H ' of the Anglo-Saxon Chronicle (150), and many others, with which scholars have long been familiar. Of great interest is the information that a fragment of the laws of Edgar has been lost in modern times (411).

There is also much to be learnt even from those manuscripts whose Anglo-Saxon content is confined to a few glosses or scribbles, for these may help us to trace the movement of manuscripts, and afford useful indications of the extent and nature of the intercourse between England and the Continent. Cotton Vespasian D. xiv ff. 170–224 (210) F. is a 9th-century Continental manuscript of Isidore, *Synonyma*, with an Old English addition of 912, thus adding to existing evidence that the generation after Alfred was not as barren of intellectual interests as is sometimes assumed. Another work of Isidore, *De summo bono*, in a 9th-century Continental hand (279), reached England by the mid-10th century, as also did the Bodmin Gospels (126), the Thorney Gospels (131), and a Continental manuscript of about 900 containing Aldhelm's *Aenigmata* and his *De laude virginitatis* (349). The date of the introduction of some other Continental manuscripts of the 9th or very early 10th century cannot be so closely established, for the entries made in England belong to the second half of the 10th century, as e.g. in Royal 15 A. xvi, of Aldhelm's *Aenigmata* (267), or even a little later, as in the Harley MS. of Alcuin's letters, which was in England (probably at York) by about 1000 (229), the Cotton Vespasian B. vi manuscript of Bede's *De temporum ratione*, in England in the 11th century (205), and the Leofric Missal (315), which was here by the early 11th century. These could have been imported after the monastic revival, as several later 10th-century manuscripts from abroad certainly were, including a 10th-century manuscript of Augustine's *Enchiridion* (278), one of John the Deacon's *Life of Gregory the Great* (311), both in England by the end of the 10th century or early in the 11th, and one containing the *Soliloquia* of St Augustine and Isidore's *Synonyma*, now at Salisbury, with English additions of the 11th century (381). But the traffic in manuscripts was not only one way. It is of course common knowledge that the Anglo-Saxon missionaries of the 8th century took, or sent for, manuscripts from the homeland, though it is not always possible to pronounce with certainty whether a particular manuscript on the Continent was produced in England or by Anglo-Saxon inmates of Continental monasteries. The Old English inscription ' Book of the Abbess Cuthswith ', probably referring to the donee in two charters of about 700 which, though lost before Wanley's time, were in the Worcester archives in the 17th century, is as unusual as it is interesting; it is on the flyleaf of a 5th-century Italian manuscript of Jerome on *Ecclesiastes*, now at Würzburg (401). Manuscripts produced in England were again crossing the Channel in no small number in the late Anglo-Saxon period. One may instance a Prudentius from St Bertin's with Old English glosses of the first half of the 11th century (7), an 8th-century *Regula pastoralis* from St Omer with 10th-century Old English glosses (369), a mid-10th-century pontifical produced in England which was at Évreux in the 11th century (370), and an 11th-century *Exceptiones de Prisciano* which was on the Continent by the 12th century (371). It would be very interesting to know by what route an early 11th-

century Boethius, written in England and given to Horton Abbey, Dorset, by a certain Aelfgyth, reached its present home in Spain (115). Though it is not news to Anglo-Saxon scholars to learn that late Anglo-Saxon culture was influential across the Channel, every new piece of concrete evidence is welcome. These exported manuscripts are normally in Latin, as one would expect. The presence abroad of a manuscript entirely in Old English, the Vercelli Book (394), has always been a puzzle, and the best speculation regards it as something of an accident. Somehow or other, a manuscript or part of a manuscript of Alfred's translation of Orosius reached Trier by the late 11th century, where the surviving leaf had some of the Old English erased to make room for local additions (391), and this shows that a vernacular text was considered useless in a foreign house.

Another, and extremely important subject which can be pursued with the aid of this book is the continued interest in and knowledge of Old English in post-Conquest times. Since Mr Ker himself collects this information on pp. xlviii–l, it will suffice here to mention the understanding of Aelfric's language in the late 13th- or early 14th-century marginalia in 21, the 13th-century collation with some other manuscript of his work in 56, 14th-century marginalia to the text of his *De temporibus anni* in 62, and the signs in 245 of careful study of the West Saxon Gospels in the 13th century. But perhaps the most interesting entry of all in this connection is in Royal 10 C. v (262) where in the margins of a 12th-century manuscript events of 1193 and 1194 are recorded in English entries reminiscent of the Anglo-Saxon Chronicle. We must now hesitate before regarding the last entry in the Laud manuscript of the Anglo-Saxon Chronicle as the swan-song of Old English historical writing.

Any student of Anglo-Saxon will realize the enormous labour that lies behind the identification of the texts and scraps of texts in this work, and all subsequent editors of and writers on Old English literature will find their work enormously lightened. If in this connection I refer only to the great advance in Aelfric studies made by the list of his *Sermones Catholici* on p. 511 ff. it is because this review is growing too long. For the same reason, a mere passing mention can be made of the contribution of this book to the history of Anglo-Saxon studies in the 16th and 17th centuries. Moreover, anyone who browses in the book will frequently come across important additions to knowledge in other fields.

For a work on this scale, performed over many years, the standard of accuracy and consistency is remarkable. It might have been preferable consistently to refer to Wulfstan the homilist as archbishop, and thus lessen the chances of confusion with the later Wulfstan, who held Worcester alone, and perhaps help to overcome the reluctance of most scholars to allow York a hand in the preservation of the homilist's work. There is surely a touch of overcaution when we are told that 119 was ' no doubt ' at Hereford when the Old English entries were added, seeing that one of them says that ' Thurkil rode to St Ethelbert's minster and had it recorded in a gospel-book ', and this remains at Hereford Cathedral to this day; and again when Worcester is said to be the probable home of 166 merely because it was bound with Worcester manuscripts, when Mr Ker's own description lends further support for this location, its agreement with a Worcester antiphonary. The argument that the Durham manuscript of Aelfric's Grammar (107) was not there in 1705 because it is not mentioned by Wanley seems to lose its force when one notes that the bilingual *Rule of St Benedict*, already at Durham by about 1100 (109), is apparently not in Wanley either. The dating of the scribbles in an Arras manuscript (4) as S. x (?) is perhaps a slip, for Max Förster in the work cited dated them about 1080, which is in closer agreement with the linguistic forms.

The Clarendon Press are to be congratulated on producing this massive work in so

convenient a format. It is essential that a work which should be under constant consultation should be stout. It is not only that; it is also handsome. Considering its magnitude, and present printing costs, it is remarkably good value for money.

DOROTHY WHITELOCK

ANCIENT MYCENAE. The Capital City of Agamemnon. *By* GEORGE E. MYLONAS. *Routledge and Kegan Paul, Ltd., London,* 1957. *pp.* viii + 201, *with* 87 *illustrations.* 45*s.*

The title of this book has been chosen to distinguish it from Professor Wace's *Mycenae: an Archaeological History and Guide.* But although the main titles are unavoidably somewhat similar and the dates of publication separated by only eight years, the scope and emphasis of each book are very different. Professor Mylonas's work is based on the Page-Barbour Lectures, which he delivered at the University of Virginia in 1955 and therefore provides the most extended account that we have so far of the exploration of the new Grave Circle outside the citadel of Mycenae, the dramatic discovery of which in the autumn of 1951 focussed the attention of the world once more on this city of Homeric legend. Three intensive campaigns (1952–54) were required to complete the excavation of this new area comprising some twenty-four graves (of which only fourteen could be considered as real shaft graves) within a circle of 28 m. diameter. Many of the burials had escaped plundering and although the ' treasures ' cannot compare in richness, variety, and quantity with those found in Schliemann's shaft graves (now referred to as Grave Circle A to distinguish it from the new, but older, Grave Circle B), the information obtained through improved methods of excavation has been invaluable. Many problems raised by the earlier excavations can now be satisfactorily explained. Many still remain an enigma, though Professor Mylonas is ever ready to offer imaginative suggestions and solutions. By careful observation during operations and intelligent study of the evidence the excavators have been able to recover many of the details of the burial customs in use during the 17th and 16th centuries B.C., the period corresponding to the end of the Middle Helladic and the beginning of the Late Helladic (Mycenaean) phases. Certain aspects of these funerary customs had been known or suspected from the results of Schliemann's earlier excavations; they have received ample confirmation and further classification in this latest enterprise. The most interesting discovery has been that of Grave Rho, the latest in the series and dating from about the mid-15th century. This was a built tomb constructed within an earlier shaft grave. It represents a complete departure from the normal type of burial and is so far unique in Greece; the nearest parallels to it are to be found in Syria.

The account and description of these graves occupy about a quarter of the book and, being the *pièce de résistance*, they are kept to the last. The earlier chapters of necessity cover a lot of old ground, but a great deal of work of one kind or another has been done at Mycenae since the publication of Professor Wace's basic work on that site in 1949 and all this is briefly summarized. There have been, for instance, Wace's recent excavations that have uncovered the houses of wealthy merchants living outside the citadel; only the basements have survived, but in the ruins of these have been gathered up the burnt fragments of exquisite ivories and—archaeologically of greater importance—the first written records to have been found at Mycenae. There has been the literally monumental work carried out by the Greek Service for the Restoration and Preservation of Ancient Monuments, which not only receives due mention in the text but is illustrated by several of the plates (or rather figures) at the back of the book. The Lion Gate is now largely restored to its erstwhile grandeur, owing in the first place to the identification by Professor Wace

of two of the fallen blocks that one time formed part of the relieving triangle. The identification is incorrectly attributed by the author to Dr John Papademetriou; to the latter, indeed, is due the credit for the raising of these blocks to their original position.

In describing and passing in review the well-known monuments of Mycenae, the Cyclopean fortifications, the palace, the houses, Grave Circle A, and the bee-hive tombs, the author has many new and original comments to offer. While acknowledging the great debt that all archaeologists owe to Professor Wace's massive and enduring labours at Mycenae, Professor Mylonas is not prepared to allow all his theories to pass unchallenged. However, the author's own alternative solutions are frequently open to as many objections as those that he brings against the older hypotheses that he seeks to displace. Certainly on one question his views appear too drastic. It concerns the chronology. Professor Mylonas would bring down the date of the Treasury of Atreus—and with it that of the Lion Gate and the Cyclopean Walls—by nearly 100 years. This reassessment seems to be based largely on the date of Mycenaean figurines, which he dogmatically asserts, ' appear abruptly about 1300 B.C.' One would like to know what his evidence is for this. The suggested date for the Treasury of Atreus is now 1250 B.C. and this same year incidentally is the revised date that is being put forward by some archaeologists for the Trojan War!

One unfortunate and recurring error calls for comment. The date of the destruction of Mycenae by the Argives is correctly given as 468 B.C. on p. 7, but thereafter it is consistently misprinted as 486 B.C. (pp. 17, 23, 122). The book contains a good selection of illustrations. Many of these are naturally familiar from previous publications, but included among them are several with a ' new look '. More than half of the illustrations—and therein lies the value—are devoted to the new Grave Circle. The book is a very useful and scholarly compendium of our knowledge of Mycenae to date.

W. D. TAYLOUR

PRÉHISTOIRE DE L'AFRIQUE DU NORD: ESSAI DE CHRONOLOGIE. *By* LIONEL BALOUT. *Arts et Métiers Graphiques, Paris*, 1955. 3500 *francs.*

It is not easy to review a book like this which all students of prehistory—the French word connotes the Stone Age only—should read carefully, even those not specially interested in North African archaeology, for it is a most stimulating work. The author is well known as Chef du Laboratoire in the Bardo Museum at Algiers and as the organizing secretary of the Second Panafrican Congress on Prehistory held in 1952. He modestly states that this book does not claim to be a ' traité de préhistoire maghrébine ' but merely a chronological essay. North Africa has been for many years a battlefield of theories and speculations in the realm of prehistoric archaeology, and it is high time to make a clear-cut distinction between fact and opinion. It is unfortunate that two other books dealing with similar matters should have been published simultaneously—Vaufrey's *Préhistoire de l'Afrique*, vol. 1 (le Maghreb), reviewed in ANTIQUITY (1957, 117), and *The Stone Age Races of Northwest Africa*, by L. Cabot Briggs (American School of Prehistoric Research, *Bulletin No.* 18, 1955). Apart from their intrinsic value both these books should be read as supplementing M. Balout's book. As was only to be expected they often represent divergent views, and it is regrettable that our author was unable to discuss them in his own.

The methodical arrangement of the book is excellent. Part I deals with the chronological foundations. There is a closely reasoned discussion of the history of the subject, of the basic archaeological and geological stratigraphy, and also of ancient climates, palaeontology (including human remains), cultural distribution and typology. Part 2 deals with Palaeolithic chronology, from the ' sphéroides à facettes ' of Ain Hanech to the Aterian complex, which in the terminology of other parts of Africa covers the Earlier and Middle Stone Ages

(including the Second Intermediate one). Part 3 (Epipalaeolithic and Neolithic) deals with Iberomaurusian and Capsian problems, and in a short but very instructive manner with the Neolithic groups and the dating of the earliest art. There is a very useful Bibliography of over a thousand entries. The publisher deserves special commendation for the excellent format and production. The plates are heliogravures of photographs of sites, fossil human remains and typical artifacts. In the body of the text are many maps and sectional drawings of excavations and the like. Drawings of implements are absent. This omission is made good in Vaufrey's book.

The author's standpoint is strongly Maghrebian. This is quite legitimate, particularly in a chronological essay. Both as a source and also as a region of passage for transcontinental movements of Stone Age cultures and peoples North Africa has played an important part in the armchair strategy of archaeologists. The author's strong dislike of that game is quite understandable, but sometimes he seems to carry his splendid isolation a little too far. Seen from the south the Maghrebian Stone Age is surprisingly African. In spite of the rareness of stratified sites, hindering the construction of a more elaborate and detailed chronology, the general rhythm of the Stone Age sequence is much more comparable with that of the regions between Cape Town and Kenya than with Western Europe. But the ' sphéroides à facettes ' found by excavation in association with Upper Villefranchian fauna at Ain Hanech are extremely important for their bearing on the problem of man's earliest artifacts; doubting Thomas himself would not deny that they are the products of human agency. The hand-axe implements of North-West Africa at this date can be divided into four chronological stages (Acheulean I to IV, treated in some of the tables as three stages only). There is a striking resemblance in the technique and form, especially of some of the ' unfinished ' hand-axes illustrated, to specimens from the southernmost part of the continent. It should be noted that the Maghreb is not, as stated by Mademoiselle Alimen in her *Préhistoire de l'Afrique* (reviewed in ANTIQUITY, 1957, 49), a province devoid of cleavers. The latest stage is equated terminologically with the Micoquian, but it would be better to confine this term to specimens more closely related to the type site. There are in many parts of the world at the end of the long hand-axe era many groups that are more or less typologically different; but future research may show that they form a distinct cultural period in more respects than one, and that the Third Panafrican Congress was right when it passed a resolution to separate the latest stages of the hand-axe complex from the Chelles-Acheul period, and to call these the First Intermediate.

In reading the book many readers will realize that in the Maghreb it is not possible to write of a well-represented Mousterian or Levalloisian stage. Except for a few sites all the subsequent material belongs to the Aterian complex. Miss Caton Thompson's famous article, which the author mentions in connection with some points, should be remembered. Long ago South African archaeologists, such as A. J. H. Goodwin, called attention to the close connection between their Middle Stone Age and the Aterian. Desmond Clark, too, noted the unexpected resemblance. The author is very sceptical of all hypotheses that involve direct crossings at this date from Africa to Spain and vice versa. I do not think that this is the last we shall hear of that problem. But before further discussions can profitably take place it would be necessary to have a summary of the Iberian Stone Age of the same kind as has been done for North-West Africa in this book. There has been much good research work by Spanish and Portuguese archaeologists since Obermaier's books appeared.

No less puzzling are the problems concerned with the Iberomaurusian and Capsian series, critically analysed in the chapters following. It is remarkable that the oldest Iberomaurusian stratum on one site lies below one with typical Lower Capsian remains,

and that the author seriously thinks that this sequence can be generalized. There is no mention of Leakey's opinion (in *Adam's Ancestors*, 1953, App. II) that the Capsian and Oranian (Iberomaurusian) should be regarded as manifestations of the same people living in a different environment. It is a pity that the author failed to make full use of the Cyrenaican material obtained by McBurney (whose name in the index is given as ' Burney, Mc '). There is little that can be added to the author's closing statement that ' le Capsien est bien une civilisation africaine ', but there still remains the problem of how the blade cultures reached the Black Continent.

The North-West African Neolithic period is an even darker one than those which preceded it. Stratified sites are alleged to be rare, and in the present state of knowledge one cannot form an adequate picture of the regional groups. The author states the facts clearly and directs the reader's attention to a forthcoming monograph on the Maghrebian Neolithic by his former pupil G. Souville. To the European reader it is important that he illustrates a potsherd which unquestionably belongs to a bellbeaker. This means that there are now three sites in North-West Africa yielding remains of this culture. This third site is not, like the other two, situated on the coast, but is about 80 to 100 miles inland from Oran. Like almost everyone else who has dealt with the problem recently, M. Balout thinks that these bellbeakers originated in Spain. He may be right but, as long as the Sahara remains an almost blank spot on the archaeological map, older opinions calling attention to resemblances between the bellbeaker culture and Nubian Group C should not be wholly overlooked or set aside. The age of art depicted on rocks, is briefly mentioned. It is remarkable that it has now been proved to have been first practised in Upper Capsian times.

What has been said above should suffice to arouse great interest in the book. Attention should be called to the fact that on the map on p. 77 the Blue Nile is misnamed the Atbara and the Sobat the Blue Nile. This book of M. Balout's, together with the others mentioned in the first paragraph above, will provide a firm basis for future research, and one hopes that it will be carried out successfully.

GÜNTER SMOLLA.

UGARITICA III. *By* CLAUDE F.-A. SCHAEFFER, with contributions by Chr. Desroches-Noblecourt, H. G. Güterbock, P. Krieger, E. Laroche, O. Masson, and J. Vandier. *Paul Geuthner, Paris*, 1956. *pp.* 303, 239 *figs.*, 10 *plates*. 4,000 francs.

This important volume consists of four distinct studies. Chapter I, which occupies more than half the book, is devoted to the seal-impressions of Hittite character found on the ' international ' tablets of the Southern Archives, together with the original seal of Mursili II found in the courtyard of the palace in 1950; there is also a short section on a pendant in the form of a triad of Hittite gods. Chapter II discusses three objects of Egyptian origin. Chapter III is concerned with fragments of tablets inscribed with Cypriot or Cypro-Minoan writing, while Chapter IV forms the first part of a corpus of bronze weapons and utensils found at Ras Shamra.

The Southern Archives of Ugarit, containing the diplomatic documents and correspondence addressed to the kings of Ugarit by the kings of Hatti and their vassals over a period of six or seven generations, constitute one of the most remarkable finds of this century, comparable to that of the Amarna letters. Dr Schaeffer entrusted the publication of the text of the tablets themselves to J. Nougayrol, but reserved the seal-impressions which they bear for a special study, to be published in Ugaritica III. Most of these seals are inscribed with the names and titles of their holders in both the cuneiform and Hittite hieroglyphic scripts and thus form a most important addition to the small body of bilingual texts available for the decipherment of the latter form of writing. As bilinguals they are

particularly valuable in dating from a period before the increasing confusion of signs containing the same consonant had resulted in the production of numerous homophones. Moreover, the seals belonging to the Kings of Hatti are particularly fine examples, both in regard to content and state of preservation, and enable us to solve certain outstanding problems left over by the more fragmentary impressions from Boghazköy. The analysis of the seal inscriptions and of the historical problems connected with them has been placed in the competent hands of E. Laroche, while Schaeffer himself has contributed an introductory section describing the seal impressions as a whole and discussing more general matters, such as the different practices in regard to the use of seals among the Hittites and their vassal kingdoms. Dr Schaeffer indeed, apparently expecting a considerable interval to elapse between the publication of this volume and that of *Palais royal d'Ugarit* IV, containing Nougayrol's edition of the tablets, has also summarized the contents of the tablets themselves which bear the seal impressions and added his own historical comments. As a result, the chapter undoubtedly gains in interest, but since the two volumes have in fact appeared almost simultaneously, there is now a certain amount of overlapping. The chapter contains photographs and line-drawings of the seal impressions, and one can only marvel at the magnificent full-plate enlargements of the seals of Suppiluliuma, Mursili II, Tudhaliya IV and Ini-Teshub (of Carchemish) reproduced on Plates I–V.

Among the most important points resulting from Laroche's analysis of the seal legends are the following. First, it is now certain that Suppiluliuma married three times and that his third wife was a Babylonian princess who, as queen of the Hittites, took the name Tawananna. Her original name, presumably that which appears in hieroglyphs beside that of Suppiluliuma on their joint seal, is not yet decipherable. This is the queen whose behaviour caused so much trouble to Mursili II that he was obliged to expel her from the palace. Secondly, the seals of Mursili II from Ras Shamra prove that the hieroglyphic name found on a seal of Urhi-Teshub at Boghazköy is to be read *Mursili*—or rather, in an abbreviated form, *Mur-li*; and a recently discovered seal from Boghazköy has confirmed that Urhi-Teshub took the throne-name of Mursili (III). Evidence is indeed accumulating that the Hittite kings, and perhaps also the queens, took new names on their accession to the throne; the Ras Shamra material provides another example in Tudhaliya IV, whose seal bears not only the normal hieroglyphic abbreviation of the name Tudhaliya (' Mount *Tu* ') but also a second name *X-Sharruma*, possibly to be read *PU-Sharruma* or *Hesmi-Sharruma*. Further examples are discussed by Güterbock in *Journal of Cuneiform Studies*, X, 120–2. As a third result one may mention that Laroche is able to present on p. 133 a definitive table of synchronisms between the kings of Hatti, Carchemish, Ugarit and Amurru in the 14th–13th centuries B.C. To enumerate in detail the wealth of epigraphical and lexicographical information extracted by Laroche from these seals would expand this review beyond due bounds.

The greater part of the second chapter is devoted to the study of fragments of an alabaster vase bearing an incised drawing of a scene which is subjected to a penetrating analysis by Mme Desroches-Noblecourt. She concludes that this design showed, when complete, a young Egyptian princess as the bride of King Niqmad of Ugarit, in the act of performing an intimate marriage rite, possibly pouring water over the hands of her royal husband. Schaeffer himself studies another interesting object, a bronze sword engraved (by a local craftsman) with the cartouche of Merneptah. He concludes that the Peoples of the Sea, who invaded Egypt during the reign of Merneptah, must have avoided Ugarit, the city remaining loyal to the Pharaoh. A third object of Egyptian origin is a marriage-scarab of Amenophis III and Queen Tiy, which is studied by P. Krieger.

Chapter III, on the fragmentary Cypro-Minoan inscriptions found on various objects at Ras Shamra, is the work of O. Masson. It appears that the Ras Shamra syllabary is not identical with any hitherto discovered. Masson is able to draw up a table of twenty-five signs, but there is no attempt at decipherment.

P. 303 contains an apology for certain minor misprints; but there are a few which the author has not detected. Title-page: for H. S. Güterbock read H. G. Güterbock. P. ix: l. 8, for chapitre III read chapitre II. P. 24: in the caption to Fig. 31, for roi d'Ugarit read roi de Karkemish. P. 55: in the caption to Fig. 76, for Trabammi read Tabrammi. One may also criticize Schaeffer's consistent designation of the elder Weather-god as ' le dieu Muwatallu-Teshub ', since *Muwatalla-* is a special epithet or title for this god, of somewhat rare occurrence. The matter is, however, of little importance, for the god is iconographically a distinct type, whatever his actual title or epithet.

It is a pleasure to review a book which presents highly important archaeological and epigraphical material in such an exemplary manner, with all the necessary scholarly discussion and comment, and with such splendid illustrations.

O. R. GURNEY

THE TESTIMONY OF THE SPADE. *By* GEOFFREY BIBBY. *Collins, London*, 1957. *pp.* 448, 63 *figures*, 32 *plates*. 30*s*. *net*.

The present extraordinary popularity of archaeology as a subject of public interest, and even of public entertainment, has recently stimulated the appearance of a number of works of popularization. These, as might be expected, have concentrated chiefly on the highlights of archaeological discovery in the field of the ancient civilizations of the Orient; and the no less fascinating story of the unfolding of the prehistory of barbarian Europe has been almost wholly ignored.

Mr Bibby has set out to remedy this lack of balance; and the success he has achieved is due not only to his considerable flair as a writer, but also to the authority with which he can speak. For, unlike many of his competitors in the current field of popularization, Mr Bibby is a professional archaeologist, an Englishman who has for many years been established in Denmark on the staff of the Aarhus Museum.

The book is divided into four parts, with the emphasis throughout on the discoveries which, in retrospect, form the foundations of the vast and ramifying edifice of modern European pre- and proto-history. The first part deals with the Palaeolithic, the second with the hunter-fisher communities of the post-glacial period, and the third with the arrival and expansion of Neolithic peoples in Europe north of the Alps. The last part, ' On the Fringe of the Great Empires ', is the least successful, and consists of a series of isolated chapters, excellent in themselves but insufficiently related, on such highlights of the protohistoric period as the Gallehus horns, the Norwegian Viking ships, Maiden Castle, Biskupin and Tollund Man.

This is essentially a book about archaeologists, rather than archaeology; and the author has an enviable gift for bringing to life, vividly and convincingly, many of the great names of the past. Moreover, in the first half of the book at least, he has also most skilfully woven into his narrative of discovery a broad sketch of the course of European prehistory, which will be absorbed almost effortlessly by the ordinary reader. Unfortunately, however, this process of painless instruction breaks down midway. The third section, dealing with the Neolithic colonizations, contains the seeds of confusion, since the primary Danubian and Western cultures make their appearance in the text only after the Beaker, Battle-Axe and Megalithic expansions, which succeed them in time; while the fourth part ignores, as if they had never been, the centuries occupied by the Tumulus Bronze and Urnfield cultures.

To make this criticism is not to belittle the book's value, not merely for the general reader, but also for the specialist to whom it is not primarily addressed. It is salutary to be reminded how great a part has been played, in the building of modern archaeology, by Scandinavian scholars, and to remember how many of them were, and are, museum officials working directly on the material in their charge. Mr Bibby records how in 1846 Worsaae, on a visit to London, expressed surprise that ' even the august British Museum, with its rich stores of classical treasures, devoted only half a room to the antiquities of its own land '. The echo of this reproach still accuses us today. Let us hope that this book, and others like it from equally qualified hands, may stimulate public opinion to silence it for ever.

R. J. C. Atkinson

CORINTH. Vol. XVI. Mediaeval Architecture in the Central Area of Corinth. *By* Robert L. Scranton. *pp.* 140, 36 *plates*, 7 *plans*. *Princeton*, 1957. $10.

The first part of this book (pp. 1–96) is devoted to a report on the excavation of numerous smaller buildings—houses, shops and an occasional church—dating from 395 to the Turkish Conquest. It is divided into six chapters, each dealing with a separate period. There is in each an introduction, in which the age is put into its historical perspective, a series of descriptions of the buildings, and a summary of conclusions. The second part (pp. 97–136) is entitled ' Architectural Analysis '; methods of construction, the architectural sculpture, and finally the plans and lay-out of the buildings are studied in turn. The second section, on the architectural sculpture, consists in great part of an analytical catalogue of the more important pieces—slabs, capitals and so on—that were unearthed; no less than 191 pieces are described and illustrated. This section is particularly important, for very little attention has hitherto been paid to such things, and the architectural sculpture is notoriously difficult to date. The author suggests (p. 103) that the results are disappointing, in that so little of the material can be dated on archaeological grounds. Yet the chapter actually represents a definite advance in knowledge, and it will only be through the assemblage of much more similar material that a reliable chronological system will eventually be established. The second part of the book is followed by two short appendices, on graffiti and on two sigma-shaped tables of marble. The form of the latter is not unusual; there are a number *in situ* in the refectory of the Lavra on Mount Athos.

It cannot be said that any of the material here published is of great importance individually. That however is due to chance. What is important is that the Christian material has been studied in detail and has been made available for purposes of comparison. This is something which is all too rare, for the later phases have seldom been so carefully and painstakingly dealt with. The organizers of the Corinth excavations are to be most heartily congratulated, and for the Byzantinist the volume comes as a worthy successor of the earlier publication on Byzantine pottery.

D. Talbot Rice

GREECE BEFORE HOMER: ANCIENT CHRONOLOGY AND MYTHOLOGY. *By* Sir John Forsdyke. *pp.* 176, *with* 8 *plates*. *Max Parrish, London*, 1956. 18*s.* 6*d.*

Three volumes are planned under this general title, the second and third to deal with Archaeology, and Language and History. This, the first, reviews the varied chronological systems of ancient historians, pointing out their artificiality and the ease with which generations and names could be supplied to fill gaps or explain apparent contradictions in the traditional accounts of Greek prehistory, of the Heroic Age. In part designed for the ' general reader ' it dispenses with footnotes and detailed references, which is a pity

as there is no comparable treatment of these problems for the English student, and the ' general reader ' is at any rate unlikely to be fooled by the illustrations into thinking that this is an easy book. The author is rightly critical and detached in his discussion, making no concessions to credulity. In fact this could have been a very important book instead of simply a useful one.

Perhaps the most we can hope for is a better understanding of the reasons why certain ancient historians believed events in their prehistory to have fallen at such a remove of years or generations. For absolute dates before the introduction of alphabetic writing, about the first Olympiad, we must wait for the archaeologist to find better dating contexts with reference to other Near Eastern cultures. Then perhaps some useful parallels might be drawn with the traditional dates of the ancient historians, but as long as the archaeologist finds difficulty in identifying the right Sack of Troy or any sort of Dorian Invasion at all, and the ancient historian fails to mention the fall of the Mycenaean palaces, hope must seem slight.

JOHN BOARDMAN

EARLY ROME II: THE TOMBS. *By* E. GJERSTAD. *pp.* 328, *with* 249 *text figures. Lund,* 1956.

This is the second of six volumes intended ' to present the entire archaeological and written evidence bearing upon the early history of Rome, from the beginning of the pre-urban epoch to the end of the Regal period, and to present the conclusions drawn from this material '. This volume describes the tombs and their contents and discusses burial customs. All finds are illustrated and drawings given where possible; a notable achievement when much of the digging was done nearly a century ago. The presence of tortoise bones in tombs reminds one of the recent finds in a shaft-grave at Mycenae and the story of Croesus' stew.

A publisher's first duty to an excavation is to describe; but his second duty, often completely shirked nowadays under a screen of technique and science, is to explain. Although Gjerstad promises four more volumes on Early Rome in which he can do this, some general remarks on the significance and dating of the finds would not have gone amiss here and would allay impatient speculation.

JOHN BOARDMAN

STUDIES IN ANCIENT TECHNOLOGY. *By* R. J. FORBES. *Leiden: E. J. Brill,* 1955–6. Vol. 1: *pp.* 194, 40 *figs.*, 9 *tables, map,* 16·50 *guilders.* Vol. 2: *pp.* 215, 38 *figs. and maps,* 8 *tables,* 19 *guilders.* Vol. 3: *pp.* 268, 46 *figs.*, 24 *tables,* 19 *guilders.* Vol. 4: *pp.* 257, 36 *figs.*, 6 *tables, map, coloured frontispiece,* 20 *guilders.*

These four volumes, which cover a large part of the field of the history of technology, and include some information on the Far East, bear witness to the immense range of Professor Forbes's knowledge.

Their main topics are as follows: Bitumen and Alchemy in Vol. 1; Irrigation and Drainage, Power, Land Transport and Road Building in Vol. 2; Cosmetics, Food, Alcohol, Preservation, and Painting Materials in Vol. 3; Textiles, Basketry, and Dyes in Vol. 4. Further volumes are promised.

A basic history of technology might describe developments in the utilization of raw materials and in the artifacts made from them. More significant on a larger scale are the effects of technological advancement upon human environments, institutions and prosperity. Professor Forbes has allowed himself—and his title justifies him—to choose freely from these extremes in his commentaries, but he is undoubtedly most enlightening on

what may be called the economics of technology, and displays very little pleasure in the description of machinery. This reviewer found, for instance, his account of the obstacles retarding the introduction of the water-mill (vol. 2, pp. 95 ff.) most valuable—and most significant, for the water-mill was the first prime mover with a power output much greater than could be obtained from human beings or animals. But none of the maddening ambiguities of medieval Arab and European engineering illustrations are resolved. De Honnecourt's absurd drawing of a saw-mill is reproduced (vol. 2, fig. 20) but not explained. And if the German 15th-century drawing in fig. 14, vol. 2 is indeed a Norse mill and not an undershot mill drawn with inadequate detail and perspective it is of a novel type worth special comment. What is described in the caption of fig. 24, vol. 2 as a water-mill is surely a pump actuated by a water-wheel (incidentally this figure, and some others, have no references in the text).

The first section of these studies reprints the author's *Bitumen and Petroleum in Antiquity*, long out of print. This work will already be well known to many readers as a most extensive examination of the early uses of the natural petroleums. A very thorough philological equipment allows the author to make full use (in all sections of these studies) of contemporary documents as well as of archaeological finds. There are many tables of technical terms in Latin, Greek, Sumerian, Akkadian, Hebrew, and Egyptian.

The considerable value of the whole section on power (vol. 2) and the interest of the calculations, probably here collected for the first time, on the power outputs of primitive machines, are vitiated by loose phrasing and definition. The phrase ' a higher level, a more concentrated form of energy production ' (p. 78) must mean a greater power output per machine; but even calculation cannot resolve the ambiguity of the sentence on p. 85 concerning the horse in antiquity: ' We know various data that it could not pull more than 62 kgr.' If 62 kg. (135 lb.) is the nett load, the statement implies that one horse could not pull one man in a chariot. This is not true of any of the ancient civilizations, and in any case disagrees with figures on p. 82 and table VII, which yield a more likely figure of about 400 lb. nett load per horse. If 62 kg. is the maximum forward force that can be exerted, the figure can again be confuted from reasonable ones on p. 82 and table VI (where ' pressure ' must mean ' force '). Whatever it is, this is a key figure for, as we are told, it kept the price of animal transport at a high level until the introduction of the horse-collar into Europe about the 10th century allowed the horse to exert its maximum pull.

The reviewer would judge that the writing of the section on cosmetics and perfumes (vol. 3) gave the author as much pleasure as any, for it makes most entertaining reading. An indignant quotation from Pliny sidelights the habits of his contemporaries: ' We have even seen people put scent on the soles of their feet . . . pray, how could it be noticed or give pleasure from that part of the body ? ' (p. 27). The illustrations to this section include two timeless portraits of Egyptian ladies applying lipstick and powder-puff (figs. 3 and 4).

The history of pigments in vol. 3 is very detailed and reliable—when read in its entirety. This qualification is necessary, for on p. 241 it is stated that ' the home-country of the real *buon fresco* paintings was ancient Crete ', and this opinion is also to be found in a table on p. 204. But in the next paragraph (p. 241) the much more convincing conclusion of Duell and Gettens is quoted, that in fact some sort of tempera was used in the application of the pigments on Cretan frescoes. In another similar case the statement that ' the mumiyâ or so-called " bitumen ", scraped from mummy linen or taken from the interior of mummies was of course nearly always a mixture of bitumen with other preservatives, etc.' (vol. 1, p. 56) is all but denied two paragraphs later: ' I will have occasion to prove that bitumen was only used in one period of Egyptian history for mummification purposes and that it is therefore wrong generally to regard the mumiyâ as bituminous material.'

There are other instances of insufficient editing. The bag-press, for instance, is described on p. 132, vol. 3, but no mention is there made of the illustrations of it which are to be found on p. 110.

The confusing problems of kermes and cochineal are well disposed of in vol. 4, pp. 100 ff.

The reproduction of illustrations, despite adequate-looking paper, is poor. It is sad to see Agricola's firm-lined wood-cuts (vol. 1) reduced almost to the size of matchboxes, and table V, vol. 3, is so reduced that some of the numbers can scarcely be read with the naked eye.

The very extent of these studies must place small faults and inaccuracies in their proper perspective. But coupled as they are with a very informal, loosely-ordered manner of presentation, the work becomes somewhat provocative. This was, perhaps, the author's wish. He has been tireless in the furtherance of scholarship in a subject which he has done as much as any man to originate, and his writings stimulate the reader to further study. Here he is exceptionally well served, for the references in all sections are bountiful, and are based for the most part on primary sources (Professor Forbes's invaluable *Bibliographia Antiqua, Philosophia Naturalis* was published in Leiden, 1940–50).

In recent years we in the West have been able to reappraise our colonial successes of the 18th and 19th centuries and thereby to realize how closely temporal conquest has been related to technological advances. The tragedy has been that the nature of our superiority has been distorted into a false attitude of racial prejudice very disfiguring to the Western character. A proper study of technology is thus of importance in the attainment of historical perspective.

It is to be hoped that the further promised volumes will soon be available. They are in all senses handbooks (each weighs under 1½ lb.) and, separately or together, should soon be found on the shelves with reference works devoted to a great variety of allied subjects.

GARRY THOMSON

LES FOUILLES D'ISHANGO. *By* J. DE HEINZELIN DE BRAUCOURT. 1957. Exploration du Parc National Albert, Mission J. de Heinzelin de Braucourt (1950). *Pub. Inst. des Parcs Nationaux du Congo Belge, Brussels. pp.* 128, 47 *plates*, 22 *text figures and* 2 *folding section drawings.* 680 *Belgian francs.*

This is a most valuable contribution to the archaeology of equatorial Africa and throws much new light on the immediately post Middle Stone Age cultures of the eastern Rift. The author is to be congratulated on a very fully documented and clear report on his excavations. These were carried out between April and July, 1950, in an old beach 9–9½ metres, above Lake Edward, at the exit of the Semliki river. Two large trenches each 150 metres long excavated at right angles to each other produced a fully representative collection of cultural material. The trenches were adjacent to the 1935 excavations of H. Dumas in which certain human mandibular fragments had been found. The human remains associated with the Ishango culture are said to belong to a robust proto-Bushmanoid stock, but to show the beginnings of certain specialization towards the Negroid. It is to be hoped that the full report on these and the human remains recovered during the 1950 excavations will not be long delayed, especially as it is only rarely that such a rich cultural assemblage is found with dated human fossil remains.

The site is dated to the period of the Makalian Wet Phase following the first Gamblian dry period. A radio-carbon date (based on shell) of 6000–6500 B.C. would seem to be quite in keeping with this suggestion. It thus falls somewhat later than the Magosian/Tshitolian industry and earlier than the Later Stone Age cultures proper, a position which would agree also with the lithicultural evidence from the site.

The site consists of a series of steadily accumulated fine gravels, sands and silts, containing, in some layers, much volcanic tuff. These deposits were laid down at the edge of the lake in fairly shallow water. The living site must have been nearby, at the top of the bank or on sudd platforms adjacent. Much of the occupation material had found its way into the water. The land fauna indicates that the country was probably somewhat more wooded and the rainfall somewhat heavier than it is to-day, while the molluscan fauna and fish show that volcanic activity was contemporary with at least the later stages of emergence of the terrace, for physiochemical changes in the water of the lake brought about by this activity caused extinction of certain forms in the post-emergence period of the beach.

The writer has divided the cultural succession into two broad divisions. The earlier is found in the shallow water beds, while the later dates to the time when the lake level had receded from the beach. The earlier cultural assemblages have been termed the Ishangian Culture and are divisible into three stages. The later assemblages belong more clearly with the Later Middle Stone Age and microlithic elements make their appearance and later become abundant. These assemblages are also, on stratigraphical evidence, divisible into three stages, the last two being associated with pottery. Cultural material belonging to Bantu occupation is found at the top. The description of the typology and technology of the lithic industries is supported by careful statistical analysis.

The stone industry at first glance is distinctly disappointing. The raw material is quartz and there are practically none of the well-known categories of tools. For the most part the tools consist of scrapers of various kinds (racloirs and grattoirs) on informal flakes and fragments of flakes. The working on these tools is such as to suggest that it results from utilization—not from intentional trimming. While it is apparent that the culture belongs with the Later Stone Age complex generally, it cannot be more closely associated either with the Wilton and related microlithic cultures, or with the Smithfield, though the bias of evidence might perhaps be slightly in favour of the latter. The stone industry from Ishango may perhaps be not unlike some of the rather amorphous industries that bridge the gap between the Magosian and the Later Stone Age proper, such as are found in the Rose Cottage Cave, Ladybrand, at Matjes River or Kisese.

What the Ishango Culture lacks in its flaked stone industry it makes up for in other ways. Numerous grindstones, pounders, mullers, dimple-scarred rubbers, pestle-stones, etc., are associated, though it is interesting to note that the bored stone does not appear before the Post-Emergence Layer (in the post-Ishango culture industry), while there is a suggestion that the polished axe appears even later still. The use of ochres for pigment is also apparent.

The Ishango Culture is chiefly notable, however, for its fine bone industry, in particular for the series of harpoons. In numbers (a total of 161 intact and identifiable harpoons) those from this site far outweigh collections from any other site. The author was able to demonstrate all the details of their manufacture from the percussion-flaked roughout made from a splinter, to the ground, polished and barbed, finished tool. Three stages of development could be discerned—the two earlier with double rows of barbs and the third stage with a single row. This is a truly fine series and the author's description is very complete. Other bone tools include awls, probable link-shafts and arrow or spear points, a gouge-like tool (perhaps a skin scraper) and bone tubes and hollow lengths of long-bone from which the epiphyses have been removed by grooving with a stone tool. The most interesting of these is a magnificent example of a ' baton ', engraved with groups of short parallel lines and with a quartz flake inserted at one end. This flake was presumably secured in position with mastic and must have formed a useful adze or scraping tool. The author discusses at some length the possible uses of the engraved lines, which can hardly have been purely to

facilitate hafting, and which look rather like the lines on Australian message sticks the use of which is simply as a guide to memory.

The author uses the harpoons to illustrate the possible associations of the Ishangian, and demonstrates with the aid of distribution maps that it was probably in the Central African lakes region that harpoons first developed and that from there the form was carried north and west to the southern Sahara and Nile Valley. The distribution of iron harpoons seems to follow fairly closely that of the bone prototype and it would be no surprise should bone examples be found far to the south of Ishango. It might be expected that they would be found in various lake areas of Northern Rhodesia, while Stow records the use of bone ' harpoons ' by Hottentots or Bushmen on the Orange River.

The makers of the Ishango Culture were hunters, fowlers and fishers, gatherers of wild melons and other fruits, and do not seem to have been averse to cannibalism. The site seems to have been a favoured camping ground and was probably occupied for much of the year, and over a sufficiently long time for the culture to pass through several stages of development.

In every way, except perhaps the price, this is a most commendable publication; the illustrations leave nothing to be desired and the identification of the stone industry has provided a valuable yardstick against which to view and identify some of the other contemporary ' indeterminate ' stone industries or other parts of the continent. It is to be hoped that other publications on the Congo Basin and Equatorial Africa generally will follow the standard set in this book.

J. Desmond Clark.

Book Chronicle—*continued from page* 79

A HISTORY OF THE ANCIENT SOUTH WEST. *By* Harold Stirling Gladwin. *pp.* 383, 175 *illustrations*, 21 *maps. Bond Wheelwright Company, Portland, Maine*, 1957. $8.50

PREHISTORIC MAN IN DENMARK. *By* Kurt Bröste, *with* J. B. Jørgensen, C. J. Becker, *and* J. Brønsted. Vol. I: *pp.* 159, *text and tables*; Vol. II: *pp.* 439, *drawings and plates.* (Vol. III will be on pathology.) *Munksgaard, Copenhagen*, 1956. 230 *crowns.*

THE POLITICAL GEOGRAPHY OF THE YUCUTAN MAYA. *By* Ralph L. Roys. *pp.* iv + 187, 17 *maps. Carnegie Institution of Washington, Pub.* 613, 1957. $1.60.

ARQUEOLOGIA DEL VALLE DE CASMA. *By* Julio C. Tello. *pp.* xxv + 344, 156 *figs.*, 33 *plates. University of San Marcos, Lima*, 1956. The first volume of the notebooks and diaries of Dr J. C. Tello, the ' Gran Maestro ' of Peruvian archaeology, who died without publishing more than a minute proportion of his vast accumulation of material and information on the antiquities of his country.

MEDIEVAL CARTULARIES OF GREAT BRITAIN. A short catalogue. *By* G. R. C. Davis. *pp.* xxi + 182. *Longmans*, 1958. 30*s.*

THE LOST GODS OF ENGLAND. *By* Brian Branston. *pp.* 191, 30 *figs. Thames and Hudson*, 1958. 25*s.*

ROMAN HISTORY FROM COINS. *By* Michael Grant. *pp.* 96, 32 *plates, end-paper map. Cambridge University Press*, 1958. 12*s.* 6*d.*

MEDIEVAL ENGLAND: An Aerial Survey. *By* M. W. Beresford *and* J. K. St Joseph. *pp.* xiv + 274, 153 *figs., including* 117 *air photographs. Cambridge University Press*, 1958. 45*s.*

HISTORY BEGINS AT SUMER. *By* S. N. KRAMER. *pp.* 333, 57 *plates*, 27 *figs. and a map. Thames and Hudson*, 1958. 35*s.* Professor Kramer of the University of Pennsylvania has spent over a quarter of a century in active Sumerological research, particularly in the field of Sumerian literature. He presents in this book a series of twenty-five essays, as he says, to present a cross-section of the spiritual and cultural achievements of one of man's earliest and most creative civilizations. The illustrations include many most admirable enlargements from the Standard of Ur and from the cylinder seals. The book has previously been published in America.

THE GOLDEN TRADE OF THE MOORS. *By* E. W. BOVILL. *pp.* 281, 8 *maps. Oxford University Press*, 1958. 30*s.* A fascinating book which was written in response to a demand for a new edition of Mr Bovill's *Caravans of the Old Sahara.* It is written, he says, for all interested in North Africa, and especially for students of West African History.

AKU AKU: The Secret of Easter Island. *By* THOR HEYERDAHL. *pp.* 367, 59 *plates*, 2 *maps*, 4 *figs. Allen and Unwin.* 21*s.* This is a popular account of his second expedition during which important archaeological discoveries were made.

DIE JÜNGERE VORRÖMISCHE EISENZEIT GOTLANDS. *By* ERIC NYLÉN. *pp.* 562, 3 *plates*, 313 *figs. Vitterhets Historie Och Antikvitets Akademien, Stockholm*, 1956. 150 *krone.*

ROMANESQUE BRONZES: Church Portals in Medieval Europe. *By* HERMANN LEISINGER. 160 *plates*, 12 *pages of text. Phoenix House*, 1958. £3 3*s.*

MYRTOU-PIGADHES: A Late Bronze Age Sanctuary in Cyprus. *By* JOAN DU PLAT TAYLOR. With contributions by J. M. Birmingham, H. W. Catling, D. H. V. Gray, M. V. Seton-Williams, Lord William Taylour and others. *pp.* viii + 118, 37 *text figs.*, 7 *plates. Printed for the Department of Antiquities, Ashmolean Museum, Oxford*, 1957.

ANCIENT SEMITIC CIVILIZATIONS. *By* SABATINO MOSCATI. *pp.* 254, 4 *figs.*, 4 *maps and* 26 *plates. Elek Books*, 1957. 25*s.* The author of this book, first published in Italian in 1949, is Professor of Semitic Philology in the University of Rome. He has completely revised and brought up to date this first English edition.

BOEOTIAN AND WEST GREEK TOMBSTONES. *By* P. M. FRASER *and* T. RÖNNE. *pp.* xiii + 229, 32 *plates. Published by Lund, for the Swedish Institute in Athens* (*Acta* 4, vi), 1957.

ARCHITECTURAL SYMBOLISM OF IMPERIAL ROME AND THE MIDDLE AGES. *By* E. BALDWIN SMITH. x + 220 *pp. and* 175 *figures. Princeton University Press*, 1956 (*London: Oxford University Press*). *Price* £3.

PICTURE WRITING IN ANCIENT EGYPT. *By* NINA M. DAVIES. *pp.* 56, 17 *plates. Published on behalf of the Griffith Institute, Ashmolean Museum, Oxford, by the Oxford University Press*, 1958. 30*s.*

ANGKOR ET LE CAMBODGE AU XVIe SIÈCLE. *By* BERNARD PHILIPPE GROSLIER. *pp.* 194, 7 *plates and maps. Presses Universitaires de France*, 1958. 1,400 *francs.* This book, the sixty-third volume in the *Annales du Musee Guimet*, is written in collaboration with Professor C. R. Boxer and describes Angkor and Cambodia as revealed by the 16th-century Portuguese and Spanish sources.

A SHORT ACCOUNT OF EARLY MUSLIM ARCHITECTURE. *By* K. A. C. CRESWELL. *pp.* 330, 72 *plates. Penguin Books*, 1958, 8*s.* 6*d.*

DUNSTAFFNAGE CASTLE AND THE STONE OF DESTINY. *By* W. DOUGLAS SIMPSON. *pp.* 136 *and* 16 *plates. Oliver and Boyd*, 1958. 18*s.*

HANDBOOK ON THE TRADITIONAL OLD IRISH DRESS. *By* H. F. MCCLINTOCK. *pp.* 28, 12 *plates. Dundalgan Press, Dundalk*, 1958. 15*s.*

BRITISH MUSEUM

Recent publications include the following:

CHRONICLES OF THE CHALDAEAN KINGS (626-556 B.C.) IN THE BRITISH MUSEUM

A study by D. J. Wiseman with the texts of all the Babylonian historical records known as the Chronicles. 1956.

Pp. xi, 99, *and* 21 *plates* £1 5*s.*

CATALOGUE OF THE MOHAMMADAN COINS IN THE BRITISH MUSEUM. Vol. II : Catalogue of the Arab-Byzantine and Post-Reform Umaiyad Coins

Descriptions by J. Walker of the Mohammadan coins with legends in various languages which were struck after the Byzantine model under the early invaders in the first century of Islam, and of all the other coins struck during the Umaiyad Caliphate of Damascus. 1956.

Pp. civ, 329, *and* 31 *plates* £5 10*s.*

CUNEIFORM TEXTS FROM CAPPADOCIAN TABLETS : Part V

Texts of 120 business documents, edited by S. Smith and D. J. Wiseman, completing the publication of the tablets of the ' Cappadocian ' class in the British Museum. 1956.

Pp. 4, *and* 50 *plates* £1 7*s.* 6*d.*

CATALOGUE OF NIMRUD IVORIES IN THE BRITISH MUSEUM

A detailed study by R. D. Barnett of the large collection of ivories from Nimrud. 1957.

Pp. 252, *and* 132 *plates* £8 8*s.*

UR EXCAVATIONS : Vol. IV : THE EARLY PERIODS

A report by Sir Leonard Woolley on the sites and objects prior to the Third Dynasty or Ur discovered between 1928 and 1934 in the course of the excavations. 1956.

Pp. 225, *and* 83 *plates* £5 15*s.*

Handbooks now available include:

Roman Coins, 5s. Antiquities of Roman Britain, 6s. Flint Implements, 4s. 6d. Inventaria Archaeologica : Bronze Age Hoards in the British Museum, 10s. Late Prehistoric Antiquities of the British Isles, 6s. Sutton Hoo Ship Burial, 7s.

Obtainable from most booksellers or from

The DIRECTOR, British Museum, LONDON, W.C.1

Lists of publications on request

TACITUS

RONALD SYME

An attempt is made here to place Tacitus in his social and political context, showing him not merely as a writer of genius but also as a senator and consul, a man familiar with empire and government. He is seen to illustrate the process that brought new men from the western provinces to success and to primacy at Rome, in politics as in literature. *Two volumes.* 84*s. net*

FOREIGN CLIENTELAE (264–70 B.C.)

E. BADIAN

The typically Roman *clientela*—that relationship between the weaker and the stronger which became the pattern of Rome's relations with weaker foreign states—is discussed in this study, which shows how *clientela* held the Empire together in the difficult times of the Late Republic. 50*s. net*

SCRIPTA JUDAICA II: QUMRAN STUDIES

CHAIM RABIN

The author attempts to create a basis for dating the composition of the Dead Sea Scrolls by comparison of certain features of the Qumran Sect with other ancient Jewish groups. The book is divided into three sections, dealing with the method of initiation, the Laws of the Zadokite Documents and certain parallels between the Qumran Sect and early Islam. (*Institute of Jewish Studies, Manchester*) 21*s. net*

THE ZADOKITE DOCUMENTS
I. THE ADMONITION II. THE LAWS

Edited, with a Translation and Notes, by CHAIM RABIN

This second edition adds parallels to the text from those Dead Sea Scrolls which were published after the first edition had appeared, as well as additional references to the Pseudepigrapha and Rabbinic literature. 25*s. net*

ESSAIS SUR DIDEROT ET L'ANTIQUITÉ

The 1955 *Mary Flexner Lectures delivered at Bryn Mawr College, Pennsylvania*

JEAN SEZNEC

The author illustrates certain aspects of the controversy over the revival of Antiquity which took place in France in the second half of the eighteenth century, and in which Diderot played a major part. '. . . wide-ranging, witty and well-documented . . .' THE TIMES LITERARY SUPPLEMENT

Illustrated 21*s. net*

OXFORD UNIVERSITY PRESS

Printed in Great Britain for ANTIQUITY PUBLICATIONS LIMITED by The Whitefriars Press Ltd., London and Tonbridge